BRAITHWAITE'S RETROSPECT.

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

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Erratum.—Page 339, line 7, omit the word "each."

SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

CONTAGIUM VIVUM.—The resemblance between a contagious fever and the action of yeast in fermentation—or the action of bacteria in decomposition—is in many points so striking that it is difficult to avoid the impression that there is some real analogy between them. It is well established by experiments, *that organic matter has no inherent power of generating bacteria, and no inherent power of passing into decomposition.* A second proposition is likewise established by these preparations—namely, *that bacteria are the actual agents of decomposition. The organisms which appear as if spontaneously in decomposing fluids owe their origin exclusively to parent germs derived from the surrounding media.* We are almost forced to the conclusion that a contagium consists (at least, in the immense majority of cases) of an independent organism or parasite. How stands this doctrine in regard to traumatic septicæmia and pyæmia? You are all aware that foul, ill-conditioned wounds are attended with severe, often fatal, symptoms, consisting essentially of fever of a remittent type, tending to run on to the formation of embolic inflammations and secondary abscesses. The notion that septicæmia is produced by bacteria, and the *rationale* of the antiseptic treatment which is based thereupon, is founded on the following series of considerations:—1. It is known that decomposing animal substances—blood, muscle, and pus,—develope, at an early stage of the process, a virulent poison, which, when injected into the body of an animal, produces symptoms similar to those of clinical septicæmia. This poison is evidently not itself an organism; it is soluble, or at least diffusible, in water, and it is capable by appropriate means of being separated from the decomposing liquid and its contained organisms. When thus isolated, it behaves like any other chemical poison; its effects are proportionate to the dose, and it has not the least power of self-

multiplication in the body. To this substance Dr. Burdon-Sanderson has given the appropriate name of pyrogen. It is the only known substance which produces a simple uncomplicated paroxysm of fever—beginning with a rigor, followed by a rise of temperature, and ending (if the dose be not too large) in defervescence and recovery. 2. We know further, from the evidence I have laid before you, that decomposition cannot take place without bacteria, and that bacteria are never produced spontaneously, but originate invariably from germs derived from the surrounding media. We are warranted by analogy in regarding pyrogen as the product of a special fermentation taking place in decomposing albuminoid mixtures, but we cannot name the particular organism nor the particular albuminoid compound which are mutually engaged in the process. 3. In the third place, we know that when a wound becomes unhealthy, as surgeons term it, the discharges become offensive—in other words, decomposed,—and when examined under the microscope they are found to swarm with organisms resembling those found in all decomposing fluids. Meanwhile the patient becomes feverish, and suffers from the train of symptoms which we call septicæmia. It is a natural inference that what takes place in decomposing blood or muscle in the laboratory, takes place also in the serous discharges and dead tissues of the wound. These become infected from the surrounding air, or from the water used in the dressings, with septic organisms; on that follows decomposition and the production of the septic poison, or pyrogen; the poison is absorbed into the blood, and septicæmia ensues. It was the distinguished merit of Lister to perceive that these considerations pointed to a means of preventing septicæmia. (Dr. W. Roberts, p. 25.)

Glandular Origin of Diseases.—The fluids secreted during various stages of some communicable diseases are capable of propagating disease, and it is proved that a definite fever can be produced in an inferior animal by the fluid secreted from a patient suffering with that fever. Dr. Richardson classes the disease produced by organic poisons as septinous instead of zymotic, preferring the word septine from this poison. The diseases thus named are small-pox, measles, scarlet fever, diphtheria, typhus fever, typhoid fever, erysipelas, hospital fever, puerperal fever, (or the fever which occurs to women in childbed), cholera, yellow fever, ague, glanders, boil and carbuncle, infectious ophthalmia. The germ hypothesis fails on other grounds than the clinical. If it were true that living germs possessing an independent

growth and vitality enter the animal body, that every disease of a communicable kind is due to its own external living germ; and that the germs continue to multiply and increase by an independent action of their own—if this be indeed true, why do the germs after a certain time cease to multiply and allow the sick person to recover? Why do they not go on multiplying until the person is infected in every part and fatally stricken? Who would get well from a disease due to living self-propagating contagions? Again, who, if the hypothesis were true, would escape fertilisation? He had noted that the number of the distinctly communicable diseases is closely related with the number of secretions. The poison of hydrophobia is from the salivary secretion; of diphtheria, from the mucous glands of the throat; of scarlet fever, he believed, from the lymphatic glandular secretion; of glanders, from the mucous secretion of the nasal surface; of typhoid, from the mucous glands of the intestinal surface; and so on. In some instances the blood itself is infected, and the corpuscular matter becomes the seat of the catalytic change. A second point that had occurred to him is that the matter or particle which sets up the poisonous action, instead of being living matter, is matter actually dead, and that its effect for evil depends, in fact, upon its being dead. He meant that dead particles of organic matter in contact with living, is the cause of the physical change which transforms the new particles of secretion into poisonous particles as they are brought up to the infected surface to be influenced by the infection. (Dr. B. W. Richardson, p. 7.)

Gout.—*Salicylic Acid.*—In acute and chronic gout the results of the treatment by salicylic acid are the most remarkable. From the commencement of my observations I was struck by the promptitude with which the most painful acute paroxysms were arrested. Within the space of two or three days the pains, the articular fluxion, the redness of the skin, and the sensibility to touch had all disappeared. Chronic gout is just as amenable to the salicylic treatment. Continued, even in moderate doses, it affords the patients absolute security from an acute attack. The tophi of the joints diminish in size, and cease to become inflamed—in a word, the cure is complete, and that without the production of any metastasis on the heart, stomach, respiratory organs, or the brain. (Prof. Germain Sée, Paris, Med. Times, July 21.)

Rheumatism.—*Salicine.*—The treatment of rheumatic fever has lately undergone a complete revolution, which has happily placed it on a much more satisfactory footing. Dr. Maclagan struck the keynote to a better mode of action by his researches

into the use of salicine. I believe the salicylate of soda will be used in all cases where the action of salicylic acid is desired. It is very soluble, which the acid is not, and it is far less liable to give rise to unpleasant symptoms. I give the preference most decidedly to the soda salt as at present advised, though it is quite possible, indeed likely, that combinations of salicylic acid with potash, ammonia, and iron, may turn out to be very valuable. In any case of articular rheumatism, whether acute, subacute, or chronic, the salicylate of soda should be tried in doses of ten, fifteen, or twenty grains, every two, three, or four hours, according to the severity of the symptoms. It is best to give it alone, or in combination with a little spirits of chloroform or syrup of orange. As a rule, the good effects of the drug are apparent after eight or ten doses; the temperature falls rapidly to normal, or even a little below, the pain and swelling of the joints disappear, and the patient is practically convalescent in two or three days; but it is better to keep up the action of the medicine for a week or so, as relapses are liable to occur if it be discontinued too soon. In some intensely rheumatic subjects it will be necessary to give it again and again before the disease is subdued, and these cases have been used as an argument against its efficacy. (Dr. J. Pollock, p. 55.)

I have treated almost every case of rheumatism, of an acute or sub-acute nature, which has come under my care since July, 1876, by means of either salicycle acid or salicylate of soda. The form of rheumatic disease for which I have most frequently ordered salicine, is the arthritic, and in these cases relief was almost invariably experienced. It is necessary for the perfect action of salicine, that the drug should be used in reduced doses for some time after acute symptoms are dispelled; I have often seen a relapse from a too early cessation of the medicine. From its greater solubility, and from its being more easily taken by the majority of patients, I have found salicylate of soda preferable to the salicylic acid. (Dr. A. D. L. Napier, p. 362.)

Salicine and its compounds is not to be regarded simply as a means of relieving symptoms, but as an agent capable of subduing the rheumatism in a marvellously short space of time. I have seen a patient who, though not suffering from a very acute attack, could hardly change his position as he lay on the sofa, perfectly free from pain within twelve hours from the commencement of treatment, and walking about his room with a firm step in sixteen hours. In many cases forty-eight hours of treatment are quite sufficient to remove the pain altogether, and most commonly marked relief is obtained in twenty-four

hours. I have generally selected the salicylate of ammonia, and have hitherto, in most cases administered it in half-drachm doses every two hours. In many cases unquestionably this quantity is much in excess of the requirements of the patient; but smaller doses are very often exceedingly disappointing, and many medical men who have tried the remedy have given it up, simply because of its failure in such small doses as have been frequently adopted. I have latterly been trying to make out for myself some indication which might serve as a guide in this respect, but have not yet had sufficient experience to enable me to arrive at any very definite conclusion. Meanwhile my impression is that the wisest course to pursue is to give the remedy in full doses at first, to watch its effects, and to reduce the amount as soon as its poisonous—otherwise called physiological—action on the brain begins to be developed. I have yet to learn that if so watched it can produce harm. It is alleged that it tends to produce delirium; but among a considerable number of patients who have taken salicylate of soda or ammonia I have had only one so affected. Some of my earliest and most valuable knowledge regarding rheumatic fever was derived from a fatal case of this kind which I had the opportunity of watching long before even the alkaline treatment was thought of, when delirium was supposed to be due to inflammation, and though bleeding had nearly passed away, leeches were still pretty freely used, to the great risk of life in such cases. I have recorded some cases of delirium occurring under the alkaline treatment. If it should be conclusively shown that delirium was more common after the treatment by salicine than after that by alkalies, it would only be to me an additional proof of the proposition that such delirium is due to depression of the nervous centres, and is to be met by free stimulation. (Dr. A. W. Barclay, p. 123.)

It is in acute articular rheumatism that the most certain and most prompt effects of salicylic acid are observed—so much so that we may promise with almost certainty the cure of febrile or apyretic acute rheumatism within a space of from two to four days. In simple chronic rheumatism the trials which I have made have proved most satisfactory. The same may be said of the acute crises which manifest themselves from time to time in simple rheumatism or in chronic rheumatic arthritis, the painful attacks of which cease as soon as in acute rheumatism. Moreover, the articular tumefactions considerably diminish, and the motions of the joints may become free even after years of pain, rigidity, and immobility—on the condition that the bony lesions have not

become too deep-seated or too advanced. (Prof. G. Sée, Medical Times, July 21.)

Typhoid Fever.—*Causation.*—It would appear as if for the production of typhoid fever poison, it were necessary that the decomposing organic matter be confined in a drain, and not freely exposed to the air. Dr. Wang, of Canton, speaking of typhoid fever, says he saw only two cases during a period of more than ten years. He has seen many cases of remittent and intermittent fevers, but never one of typhoid. It may therefore be safely affirmed that this disease is not at all prevalent, although we should expect a different state of things, as the causes that are usually supposed to produce typhoid fever are in full operation. In Canton large numbers of the natives are daily using water and inhaling air charged with the impurities of human excreta, apparently with utter impunity. River water is greatly used, and that used by the boat population along the different jetties is extremely filthy, and must be largely contaminated with human and other impurities. They do not suffer from diarrhœa and fever more than others, but rather less. The filthiness of the creeks which ramify into different parts of the city are much worse than this. He gives one illustration of a creek near the foreign settlement which has been under his observation for some years. It is narrow, crowded with boats—innumerable houses on each side—the alvine dejections and other impurities of thousands of inhabitants along it are daily discharged into the stream, yet the water, too dirty even for washing, is daily used for culinary purposes without being filtered, or is precipitated with alum as is done elsewhere. Here we should expect the prevalence of such diseases as typhoid and diarrhœa among the inhabitants occurring often enough to excite attention, but their very impunity is one of the reasons for their continuing to use the water. He adds, a detailed examination of this creek and the disgusting habits of the inhabitants would almost unsettle one's ideas of the connection between typhoid fever and polluted water. (Dr. J. Dudgeon, p. 43.)

Antipyretic Treatment.—In cases of enteric (typhoid) fever attended with hyperpyrexia, the use of the bath is imperatively demanded. The temperature of the water should vary according to the condition of the patient, those especially feeble or with weak hearts requiring a temperature of 80°, which must subsequently be reduced to 70° or 65° , if the patient stand the bath well. The majority of patients are placed in a bath of 60° to commence with. The temperature of the patient is taken in the rectum, and his pulse noted immediately

before immersion. He is then wrapped in a sheet and lifted into the bath. Another sheet is then stretched over the bath, and the one surrounding the patient withdrawn, so that the water circulates freely round his body. This is a matter of some importance, for if the sheet be left round the patient, the water inside it soon becomes warm, and the cooling process does not go on. His head and face are immediately sponged, and as much of his head kept under water as can be managed without the water being permitted to enter his mouth and nose. His tongue and mouth should be cleaned at the same time. In those cases in which the patient has been placed in a bath of 80°, the temperature is reduced to the point required. The length of time he remains in the bath depends on the height of his temperature and that of the water. If the water be at 60°, from fifteen to twenty minutes will be sufficient, but in those cases where it is 80° to commence with, and has subsequently been reduced to 70°, half an hour's immersion is required. While the patient is in the bath, his bed is being prepared for his reception. A mackintosh is spread over the bottom sheet, and over this a blanket. When he is removed from the bath he is, at the moment of being lifted, covered with the sheet previously stretched over it, and is then placed on the bed and wrapped in the blanket on the mackintosh, the wet sheet at this moment being withdrawn. In this manner the patient may be bathed without the least exposure of his person. He is left for half an hour in the blanket, at the end of which time it and the mackintosh are withdrawn. Occasionally a little brandy, before and during the bath, is given; but this of course depends on the condition of the patient, and is required only when he is very weak. Nothing can be more striking than the influence of the bath on the nervous system. Although unconscious or delirious before the bath, after about a quarter of an hour's immersion the patient usually recovers consciousness. Nor is this the only beneficial effect; the bath is in a large majority of cases followed by quiet sleep, which may last an hour or two, or more. (Mr. S. F. Murphy, p. 38.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Angina Pectoris.—*Nitrite of Amyl.*—We have as yet no data which will enable us to give a complete explanation of the pain of angina pectoris. Whether it be a pure neuralgia with consequent inhibition of the cardiac contractions, or a result of cardiac muscular cramp, or of overdistension of one or more of the heart's cavities, or a combination, in some cases

at least, two or more of these conditions, must remain at present a matter of doubt. It is agreed on all hands, that the pain is often agonising, and that it often radiates into neighbouring sensitive nerves, more especially those of one or both arms. The remedial efficacy of the nitrite of amyl is due to its remarkable influence over some forms of neuralgia, and not to its relaxing effect upon the arterioles, except in so far as its antineuralgic power may depend upon its influence on the arterioles. One of the most striking examples of great and permanent relief by the use of nitrite of amyl is afforded by the case of Dr. Madden of Torquay, who states, that for a considerable time he had hesitated and neglected to employ the nitrite of amyl, in consequence of his belief that it was suitable only in those cases in which the face is pallid during the paroxysms. "As mine was flushed, I dismissed from my mind all thoughts of trying it and paid the penalty of hasty conclusions in the shape of a large amount of acute suffering." The result of a first trial of five drops inhaled during a severe paroxysm "was truly wonderful. The spasm was, as it were, strangled in its birth; it certainly did not last two minutes instead of the old weary *twenty*, and so it continued. The frequency of the paroxysms was not diminished for some time; but then they were mere bagatelles as compared with their predecessors. Under these improved circumstances, strength gradually retnrned, the attacks became less and less frequent, and finally ceased." (Dr. G. Johnson, p. 61.)

FACIAL NEURALGIA.—*Nitrite of Amyl.*—I was consulted by a lady who for a fortnight had been suffering from severe facial neuralgia, which had resisted various remedies that had been employed before she came under my care. I advised her to drop five minims of nitrite of amyl on blotting-paper and to inhale the vapour. The pain was rapidly and completely subdued. In a few hours it returned in a milder degree, and it was again promptly removed by the amyl vapour, and, after three or four repetitions of the dose, the cure was complete. (Dr. G. Johnson, p. 64.)

HYSTERIA.—Moral treatment is the method eminently rational and successful, whilst that which is too often adopted is a system most perfectly devised to perpetuate the hysterical state. By regarding it as a positive disease and fostering this idea by constant medical attendance, by nursing and the administration of medicine, a mode is adopted exactly suited to convert a functional trouble into a real malady The speedy cures which occur in hospitals amongst patients who have had nervous maladies of months' and even years' duration

contrasts most remarkably with the failure of treatment in like cases in private practice. The whole question of pain from a clinical point of view is one of the greatest importance; indeed, a large part of one's daily occupation is the attempt to unravel the meaning of pain and discover whether it be really objective and have an actual cause in the painful spot or whether it be altogether subjective. In a state of health a man should have no knowledge of the working of his animal machinery, but as soon as he gets out of gear he becomes conscious of his head, his heart, his stomach, and other organs, and moreover he begins to experience aches and pains. He has headache, or backache, pain in the side, or pain in every part of the body. In the condition known as hysteria, there may arise neuralgia, myalgia, enteralgia, and every other algia. In various material diseases the same, however, may be seen, as in poisoning by lead and in alcoholism, where there is not a part of the body in which pain may be not experienced. In some instances of the kind there may be an actual change in the nerve, but in others the cause can only be attributed to a morbidly sensitive state of the centres. (Dr. S. Wilks, p. 65.)

INSOMNIA.—Ordinary cases of insomnia may be divided into three classes—senile, toxic, and psychical. In the senile form of the affection, the disorder depends upon degeneration of the cerebral arteries, and is difficult of cure; in the toxic, upon abuse of alcohol, tea, or tobacco, and ceases upon the removal of the cause; in the psychical, it arises from continued and excessive mental strain, grief, anxiety, worry, &c., and is usually successfully treated by full doses of bromides conjoined with tincture of ergot and cod-liver oil. If the insomnia be serious, it must be stopped at once by hypnotics, preferably by opium. (Dr. Sawyer, Brit. Med. Jour., July 14.)

SATURNINE PARALYSIS.—*Diagnosis by Electricity.*—A case of obscure paralysis of both arms and legs, but most marked in the hands and wrists was subjected to electrical examination, with the following results:—The strongest induction current he could tolerate produced no effect on the extensors of the wrist or fingers, but the supinators reacted fairly, as did also the long abductor of the thumb (extensor ossis metacarpi). Tested with a 20-cell voltaic current (Gaiffe-Clamond), the extensors of the fingers reacted slightly but distinctly, especially with a descending current, and interruption of the negative pole. My suspicions were at once aroused by these results as to the cause of the paralysis. No form of progressive muscular atrophy, of traumatic or rheumatic paralysis, or of central nervous disease which I had seen,

presented similar features, and the only explanation which the symptoms seemed to admit of was that they were *due to lead.* On questioning the man he admitted the habit of chewing a bit of lead "instead of tobacco." There was a well marked blue line on the gums. The ordinary treatment for lead poisoning resulted in a cure. The grounds upon which the primary diagnosis of saturnine poisoning was based were shortly these:—(*a.*) The special implication of the extensors of the wrist and fingers, as shown by their loss of power, their atrophic condition, and their refusal to respond to faradisation. This latter feature is, I think, invariably met with in true lead palsy. (*b.*) These muscles retained some degree of excitability, although a diminished one, to an interrupted voltaic current. In extreme cases even voltaic irritability is lost; and it is certainly incorrect to state absolutely, as some writers do, that, in lead palsy, irritability towards a slowly interrupted voltaic current is always *increased.* (*c.*) The comparative exemption of the supinators. This is a curious, and at present, inexplicable fact in relation to the toxic action of lead, and one which I have repeatedly verified. (*d.*) The immunity enjoyed by the interossei and lumbricales. It is only in severe phases of lead poisoning that these muscles are seriously impaired; whereas in Cruveilhier's disease they are commonly engaged, and indeed sometimes are the first tell-tales of the inroads of the palsy. (Dr. W. G. Smith, Dublin Jour. of Med. Science, May.)

Tetanus.—While all the fatal symptoms disappear upon the inhalation of chloroform, they return at its removal with unabated violence, and the disease generally comes then to its fatal conclusion without delay. Chloroform is now very rarely administered in tetanus, but chloral hydrate and calabar bean, the drugs which most nearly approach it in respect of physiological action, are much in favour. Eilert pretends that of opium, chloroform, chloral, curare and calabar bean, the latter is the only one that acts satisfactorily on the spinal cord. He recommends either previous narcotism by chloral, or the simultaneous administration of atropin, so that both of these combinations have probably been tried. The effect of the bean seems to be all that one would expect from its known properties; and that it is indicated the large doses requisite to obtain its physiological action seem to prove. The spasms are controlled and the body heat sinks, and if the drug be withheld the paroxysms return, while if it be pressed the patient comes into a somewhat dangerous condition. Those who have used it recommend its hypodermic injection, not less than $\frac{1}{3}$ grain of the extract every two hours,

and so large a dose is required to procure contraction of the pupil that it has even been seriously contended that its effect in subcutaneous injection is to dilate it. (Editor of Practitioner, p. 59.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Gouty Phlebitis.—During an attack of acute gout the venous blood is thoroughly impure. Its noxious properties attest themselves *generally* by vascular excitement, with malaise or pyrexia; *locally* by an exaltation of the same condition to a state of extreme tension in the toe; whilst the superficial veins, unless these are, as is sometimes the case, but very feebly developed, are notably bloated over the goutily-affected textures and for some distance up the leg, sometimes even to the thigh. Gouty phlebitis, on the other hand, manifests itself in the vein coats, irrespective of any particular locality; is attended with an amount of constitutional disturbance much in conformity with the degree of the concurrent local irritation; and the blood is, I believe, not known to be ureic to a poisonous or hurtful extent. These differences entitle this latter disease to a distinct place in nomenclature, but only on condition that it has an etiology as well as a pathology, in concurrence with its symptoms, peculiarly its own, and in very limited affinity with those of acute gout. Whilst, then, gout is due to blood poison, gouty phlebitis is or may exist entirely independent of it. It is due to a gouty diathesis or constitution, hereditary or acquired, by which a free elaboration of urea or its salts may take place in the blood, and cause an acute paroxysm; or, without any such blood impurity, may give rise to local inflammatory disorders on comparatively slight provocation, especially in the vein coats, whose morbid propensities make them a comparatively easy prey to inflammatory action from a variety of constitutional causes. (Mr. J. Gay, p. 191.)

Popliteal Aneurism.—*Esmarch's Bandage.*—If on further experience the treatment of popliteal aneurism by Esmarch's bandage turns out as successful as in the following case, it will supplant all our present plans of treatment of that disease. The swelling had only existed three weeks and was increasing rapidly — pulsation was forcible. The limb was rolled in a flannel bandage from the toes to the lower part of the popliteal space, and again from above the aneurism to the groin. Esmarch's india-rubber bandage was then applied, with only moderate firmness, from the toes to the aneurism, the patient being in bed; he was then made to stand up until the sac

was well filled with blood, when the elastic bandage was applied from above the aneurism to the groin, where the limb was surrounded with the thick india-rubber tubing so as completely to arrest the circulation in the limb. The aneurism and popliteal space were thus left exposed, so that the least pulsation in the sac could be detected. In this way the circulation was stopped for one hour, during the last half of which chloroform was used on account of the pain. At the end of the hour, while the patient was still under chloroform, Esmarch's bandage was removed, and the Italian tourniquet was applied to the femoral and maintained in position for two hours. At the end of the first hour of the tourniquet pressure the patient began to complain of intense pain, and shivered a good deal; feeling, as he expressed it, "thoroughly cold." At 6.10 p.m., when three hours had elapsed, the tourniquet was removed on account of the intense pain, the patient refusing to take any more chloroform and being unable to bear the pain. The aneurism was found to be solid, and about half the size it had been at the commencement of the treatment. Towards night the patient complained of pain in the limb, and his temperature rose to 100·2°. Next morning he was still suffering from the effects of the chloroform, but had lost all pain in the limb, and in other respects was quite well. He was discharged from the hospital on April 6th, with the aneurism cured. (Mr. T. Smith, p. 188.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

HOWARD'S "DIRECT METHOD" OF ARTIFICIAL RESPIRATION.—Strip away his wet clothing to the waist, and of it make a large, firm, solid bolster. Quickly turning his face downward, the bolster beneath the epigastrium, making that the highest point, the mouth the lowest; placing both hands upon his back immediately above the bolster, my whole weight is thrown forcibly forward, compressing the stomach and lower part of the chest between my hands and the bolster for a few seconds two or three times with very short intervals. Quickly turn the patient on his back, the bolster beneath it making again the epigastrium and anterior margins of the costal cartilages the highest point of the body, the shoulders and occiput barely resting on the ground. Seize the patient's wrists, and having secured the utmost possible extension with them crossed behind his head, pin them to the ground with your left hand, so as to maintain it. With the right thumb and forefinger armed with the corner of a dry pocket-handkerchief, withdraw the tip of the tongue,

holding it out of the extreme right corner of the mouth. (This is the easiest, least barbarous, and firmest way of holding the tongue.) If a boy be at hand, both wrists and tongue may be confided to his care. To produce respiration, you now kneel astride the patient's hips, rest the ball of each thumb upon the corresponding costo-xyphoid ligaments, the fingers falling naturally into the lower intercostal spaces. Now, resting your elbows against your sides and using your knees as a pivot, throw the whole weight of your body slowly and steadily forward until your mouth nearly touches the mouth of the patient, and while you might slowly count one —two—three; then *suddenly*, by a final push, spring yourself back to your first erect position on your knees; remain there while you might slowly count one—two; then repeat: and so on about eight or ten times a minute. (Dr. Benj. Howard, Vermont, U.S., p. 95.)

Nasal Polypus.—Small polypi may be painted frequently with strong astringent solutions, (Pigmenta, T. H. Ph.), and chloride of zinc or perchloride of iron is well adapted for this purpose. The author has found the best results from the use of the latter salt, mixed with just sufficient water to form it into a thick paste. Bichromate of potash and nitrate of silver have also been tried frequently with more or less success. Mr. Bryant thinks highly of the insufflation of finely-powdered tannin into the affected nostril. No method of removing mucous polypi is equal—in simplicity, certainty, and rapidity —to evulsion by means of forceps. For the purpose of snaring polypi with a noose of string, and then employing traction, the instrument invented by Mr. Hilton may often be employed with advantage. In using this snare the polypus is embraced as near as possible to the base of the pedicle by a loop of wire, which is then drawn home and tightened by means of a cross-piece sliding on the stem of the instrument. Sufficient force is then applied to tear the growth from its attachments. The difficulty consists in getting the loop of wire over the body of the tumour and well round the peduncle. This object may occasionally be effected by the aid of a small blunt fork, which being passed into the nostril, can be made to direct the loop over the thick extremity of the growth. (Dr. Morrell Mackenzie, p. 201.)

Phthisis.—In phthisis, as in all other diseases, although chance remedies may in rare cases be hit upon, a genuine curative *method* must depend upon a close insight into cause and effect —into primary causes, into subsidiary causes or conditions, and into the products of these factors severally and together. If, then, phthisis be a disease in which a primary lesion

generates blood-poison, and this poison is found to set up adenoid and septic poisoning, the treatment must, putting it briefly, be antiseptic. The radical treatment of phthisis pulmonalis, would, therefore, unquestionably be to cut down upon the ulcers or abscesses, and to cleanse and drain them. We might thus hope to dry up the poison at its source, when, as unquestionably, the results of it would tend to subside, unless so far gone as to amount to substantial mischief in themselves. Analogy would lead us to expect what practice shows us—namely, that simple septic fever would cease at once when the source of the poison is cut off, for it cannot be too earnestly impressed upon all physicians that septicæmia is, as a rule, a daily cycle, not the result of one, but of repeated absorptions; that, in fact, it is a disease bringing about its own daily cure, but day after day reproduced. Cleanse the uterus, cleanse the vomica, drain the empyema, wash the stump, open the abscess, sponge out the peritoneum, syringe the ears, swab the throat—in a word, remove or disinfect the poison at its source,—and in nine cases out of ten the hectic fever will cease within twenty-four hours, not to return. Hope seems to lie in some inhalation plan, or in some filtering of the respired air, but here one meets with much disappointment. It may be that inhalations do not reach the part, or that their action should be quite continuous. Can we then, in default of success with inhalations, and with respirators, put the patient into a permanently aseptic atmosphere? If into a natural atmosphere so much the better. We have learnt much of late concerning the septic contents of atmospheres, and we know that in towns and foul dwellings or workrooms an aseptic atmosphere cannot be found. We carry, then, our test infusions into the country and away from hay chambers, &c., and there we find them more steadfast, and more steadfast out of doors than within. The higher we carry them the steadier they are, and at an elevation of 5,000 feet they positively may remain unchanged for weeks, unless purposely infected. At Davos meat when hung up does not putrefy, but is thus dried and kept for use, and test infusions remain stable for long periods (Waters, loc. cit.) Nor do the lungs there rot in the living man. Catarrhs and pneumonic inflammations are common enough, but the morbid secretions do not foul, and phthisis is therefore unknown. Davos is an upland valley in the Grisons, at an elevation of about 5,150 feet above the sea. (Dr. Clifford Allbutt, Lancet, Oct. 20.)

Renal Asthma.—There is a curious form of asthma liable to occur in Bright's disease. It is not persistent, but occasional and intermittent, coming on in paroxysms, in some cases

soon after food has been taken, and especially after an evening meal; while in other instances it occurs mainly during the night, when it entirely prevents or greatly disturbs the patient's sleep. In some cases the attack resembles one of spasmodic asthma, and there are loud sibilant râles over the lungs, apparently the result of bronchial spasm; but in the more typical cases the phenomena are quite different. The breathing is hurried and laborious, the heart's action is rapid and feeble, and there is more or less lividity of the lips and of the general surface; yet, on auscultation, loud puerile respiration, unmixed with râles or crepitation, may be heard over the whole of both lungs; while in other cases, especially after a long continuance of the dyspnœa, fine crepitation may be heard over the bases of the lungs, but there is obviously no deficiency of respired air, nor any change in the physical condition of the air-passages to explain the distressing symptoms. It can scarcely be doubted that the symptoms in question are the result of a form of uræmia. The proximate cause of this uræmic dyspnœa is probably an arrest of the blood current through the pulmonary capillaries, occasioned by spasm of the pulmonary arterioles caused by the influence of the impure blood upon the vaso-motor nerves and centre. (Dr. G. Johnson, p. 97.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Cleft Palate.—Operations on very young children are, as a rule, extremely unsatisfactory, and this is the experience of other surgeons. Unless there be good reasons for doing it, the operation should not be undertaken before the age of five or six. If the wound bursts open, the surgeon should never despair of getting considerable, if not complete union, provided the smallest portion of the edges can be got to adhere. The persevering application of strong nitric acid will promote granulations, and I have seen surprising results in cases which I at first regarded as hopeless. It is well to bear in mind that ether excites the salivary secretion. I therefore prefer chloroform in all operations about the mouth. (Mr. F. Mason, p. 195.)

Hemorrhoids.—In considering the treatment of hemorrhoids, bear in mind this standard rule, that whatever you remove as *bonâ fide* integument may safely be done by a curved pair of scissors; whatever is removed as *bonâ fide* mucous membrane must be done by clamp and cautery, or by ligature. In the out-patient department, I constantly cut off cutaneous tags around the anus with scissors, and treated the owners

of them as out-patients; but never did one venture so to handle hemorrhoids. Patients with piles require careful rest and attention prior to operating. Mr. Henry Smith has introduced a pair of forceps for holding these slippery customers, and then burns them off; my own experience inclines me to hold that the whole curative credit is due to the cautery. At this hospital we use a much stronger clamp than Smith's, and more after the St. George's Hospital pattern, because the grip is much firmer and cannot slip. The cautery I now use is this very ingenious and practical platinum cautery invented by Dr. Paquelin; after heating the platinum-tube over a spirit-lamp, the vapour of benzoline driven over this heated surface maintains its temperature as a cauterising agent. The late Mr. Bruce devised an arrangement for utilising common gas for cautery purposes, but benzoline is more portable than gas. No modern invention has succeeded so well in neutralising the apparent brutality of the hot iron as this of M. le Docteur Paquelin. These rectal wounds must all be treated by consummate cleanliness, and require no other dressing. You will find the steam-spray very grateful, and a soft hog's bristle brush an useful adjunct. (Mr. R. Davy, p. 212.)

The two operations at present in general use for the cure of internal piles are—1. The operation by ligature; 2. The operation with the clamp and cautery (Smith's). I can offer some strong evidence in favour of the clamp and cautery in connexion with the amount of pain and irritation following the operation, and the quickness of recovery after it—for, in three of my cases operated upon in this way, the patients had previously undergone the operation by ligature. The testimony of all these patients who had experienced both methods was most strongly in favour of the clamp and cautery. If the clamp and cautery are used for the removal of internal piles, it is very important that the cautery or other heated instrument should be carefully applied, and at an almost black heat. I have recently employed the thermo-cautery knife in two cases to cut off the piles after they have been seized with the clamp, and I have found it most simple and efficient in its application. As is well known, internal piles are often complicated with external piles, or with a looseness or redundancy of the skin round the anus,—and it becomes a point of considerable practical importance to consider how far such complications should be dealt with when operating upon the internal tumours. When distinct external piles exist along with internal ones,—there can be no doubt that the proper practice is to cut them off at the

time of operating upon the internal tumours; but, when the condition is simply a general looseness of the skin surrounding the anus, then I think that it should not be interfered with, unless it is very marked. (Mr. T. Annandale, p. 213.)

Operations on the Rectum.—In all operations on the rectum, attend to the following hints:—2. That the bladder be empty; 2. That the banks of the sewage-canal be well washed previously with tepid soap and water; 3. That your own hand be oiled, the nails cut short, and the semilunar folds around the nails filled with soapy smears; 4. That suitable lavatory accommodation (with disinfectants) be ready for your own ablution. Divide the fistulous tract accurately; retraction of the sphincters results; the floor of the sinus is free from friction, and granulations adhere from the floor towards the circumference of the anus. Administer opium *per rectum* or by the mouth as occasion seems fit, so as to quiet the intestinal tract. Nitrous-oxide gas may well be used for these short operations, because sickness is a very aggravating occurrence as a sequence. (Mr. R. Davy, p. 212.)

Strangulated Hernia.—Your object is by gentle pressure to empty the bowel of air and fluid, to empty the vessels of blood, and to push back some of the contents of the hernial tumour through the ring; and the first thing that enables you to be certain that you will succeed in that, is feeling a little air or fluid gurgling back into the cavity of the abdomen. Taxis is very much assisted by chloroform, and I think much more assisted by chloroform than by ether. I do not myself recognise that very great difference in the danger of anæsthesia from chloroform and from ether which some surgeons profess to feel. It seems to me that complete surgical anæsthesia is always a dangerous thing, whether produced by chloroform or ether; and I think chloroform produces so much more complete relaxation, and that so much more speedily, that it is very much more convenient in the reduction of dislocations and strangulated hernia. It seems to me that when vomiting is distinctly declared, and has turned from bilious vomiting into vomit mixed with the contents of the small intestine—what is sometimes called fæcal, although it is not fæcal, but where the vomit has turned from the green colour of bilious vomiting to the dirty brown of the contents of the small intestine—then you had better not wait any longer; if you cannot by a renewed attempt at taxis reduce the hernia, you must operate. But that is an interval which allows you a considerable amount of time. You do not, usually speaking, get that condition of things until after twenty-four hours of strangulation, and if you have the

patient under your care during the whole of that time there is no reason why you should not put him once, if not twice, under the influence of chloroform, and attempt taxis. (Mr. Timothy Holmes, p. 208.)

AFFECTIONS OF THE URINARY SYSTEM.

Albuminuria. — Under ordinary circumstances albuminous bodies do not diffuse through animal membranes, but albumin of eggs will pass out through the kidneys, while the albumin of blood does not do so under similar conditions. It seems curious that one sort of albumen should pass through the vessels of the kidney, while another should be retained, and the only feasible explanation of it seems to be that the molecule of the different albumens varies in size. For the passage of substances in solution through membranes has been shown by Moritz Traube to resemble very much the passage of powdered matters through a sieve. When the particles of the substance are too large to pass through the meshes of the sieve they are retained, but when they are too small they pass through. Thus almost all crystalline substances readily diffuse through animal membranes, and Graham divided bodies, according to their diffusibility, into crystalloids and colloids. But there is one marked exception to the rule that crystalline bodies diffuse, and this is the colouring matter of the blood, hæmoglobin. This substance differs from most other crystalline bodies in possessing a very high molecular weight, and the molecule is therefore, in all probability, very large. Traube's hypothesis at once explains this curious exception to Graham's law, and renders it probable that hæmoglobin does not diffuse simply because its molecule is too large to pass through the pores of ordinary membranes. True albuminuria consists in the passage of serum-albumin, which is a normal constituent of the blood, into the urine. It depends either upon alterations in the structure of the kidney, or interference with the circulation through it, or upon both. Pathologically we find albuminuria occurring from venous congestion, in cases of thrombosis of the vena cava, in mitral disease, and in emphysema whenever the right ventricle begins to yield and allow regurgitation into the veins. There is, however, another cause of venous congestion which at first sight would appear likely to have an effect exactly opposite to that which it actually produces. This is obstruction to the flow of blood through the renal artery. (Dr. T. Lauder Brunton, p. 124.)

Arsenic.—Dr. Brunton relates a case in which his attempts to act upon the vessels of the kidney having been futile, and

treatment with mercury and quinine having also been useless, he determined to try to act upon the secreting structures of the kidney, and accordingly gave arsenic, which has a considerable action upon tissue change, and appears to possess also a special affinity for epithelial structures. The patient took three minims of Fowler's solution at meal-times. Almost at once the albumen disappeared, and the patient was able to do much more work than usual without bringing it back. The following points are brought forward in connection with this case:—1. The intermittence of the albumen and its connection with the digestion; 2. The absorption of albuminous matters from the intestine as a cause of albuminuria; 3. The utility of arsenic as shown in this case; 4. The possible use of pancreatine; 5. The effect of quinine in increasing the albumen. (Dr. Lauder Brunton, p. 128.)

Hydrocele.—Acupuncture is eminently successful, especially in young children; the hydrocele is punctured in two or three places with a broad needle, the flat of the needle being turned at right angles to the puncture before being withdrawn, allowing the hydrocelic fluid to percolate into the cellular tissue external to the tunica vaginalis, substituting "an anasarca of the scrotum for a dropsy of the tunica vaginalis," the fluid being subsequently removed by absorption. Of iodine injected some prefer a solution, others ʒ ij of the pure tincture; whichever is used does not apparently much matter, the chief point being the manipulation of the sac so that the injection may be brought thoroughly in contact with the whole of the internal lining of the tunica vaginalis. Another point of importance should be remembered, and that is that a platinum canula be used, as the ordinary silver canula is destroyed by the action of the iodine. (Mr. S. Osborn, p. 240.)

Incision of Hydrocele Antiseptically.—In certain cases of hydrocele the following plan of treatment is very successful. It is indeed absolutely certain to effect a cure. It is suitable when treatment in the ordinary way by iodine injection has failed, or where the latter and usual plan may be contra-indicated by ill-health or age. The only preliminary required is to shave the pubes and scrotum on the affected side; an incision is then made with the ordinary antiseptic precautions over the upper part of the hydrocele downwards for a distance varying with its size, one of about two inches will usually be sufficient. This incision should go at once down to the tunica vaginalis, which is nearly always thickened, and readily recognised. A corresponding incision is then made through the tunica vaginalis, and this should be done quickly; otherwise, as the long-stretched dartos rapidly con-

tracts, the resulting aperture will be found too small for the satisfactory carrying out of the next step. This consists in tucking in a narrow strip of gauze soaked in carbolic oil. The object of this is to ensure that granulations shall spring up all over the opened sac so as to procure its obliteration. The usual antiseptic dressings are then applied, overlapping the wound carefully in every direction, perforated for the passage of the penis, and kept in position by a double spica bandage. The wound should be dressed once in the two following days, after this every second, third, or fourth day. All that is required is to syringe out the wound with a solution of carbolic acid (1 in 40), and on the first one or two occasions to reinsert a fresh strip of oiled gauze. Four or five days after the operation the patient may get up, and at the end of a fortnight he may leave the hospital. If the wound has not completely healed by this time all that will be left will be a small granulating surface, and the only treatment needed is to reapply every other day a bit of borailic lint, and to keep the testicle suspended a little longer. (Dr. W. H. A. Jacobson, p. 236.)

Lithotomy. — *The Rectangular Staff.*—The rectangular staff originally introduced by Dr. Buchanan possesses many advantages. It is bent at right angles three inches from the point, and is hence "rectangular." It has a lateral groove along the horizontal part, and the end of this groove is closed. When the instrument is introduced, the angle lies in the membranous part of the urethra, close in front of the prostate gland, and can be felt by the finger placed in the rectum, or by a little pressure on the perineum, to occupy a point a little in front of the anus. The horizontal part lies parallel to the rectum, and extends into the bladder. In operating, the staff is so held as to occupy an intermediate position between being hooked up under the pubis and being pressed down on the perineum, and the operator keeps it steady and distinguishes its correct position by placing his left fore-finger in the rectum under its horizontal part. The thumb of the left hand is at the same time pressed gently in front of the anus, so as to mark the site of the angle and to keep it steady. The exact position of the angle is very easily determined, as there are only the skin, superficial fascia, and some fibres of the sphincter between it and the thumb. The knife used is a straight-backed one, whose blade exceeds in length the grooved portion of the staff by about one-fourth of an inch. The point is sharp, and it should have a cutting edge on its back for about half its length, by which the tissues along the groove are more

surely divided towards the middle line of the perineum. The shoulder of the knife is low, and the breadth of the blade equal from shoulder to hilt. When the patient has been tied in the usual position, and the staff placed as above described and fixed by the operator's left hand, the knife (held short and above the hand, palm upwards) is slowly inserted close above the anus, "just where the mucous membrane shades into skin," and close to the raphé. The edge is turned to the left side of the perineum, or to the operator's right. The blade is not introduced parallel to the horizontal part of the staff (which would greatly increase the risk of its escaping from the groove as it passed on into the bladder), but obliquely, so as to impinge on the groove at an angle; and as it is afterwards pushed on towards the bladder, a *slight* obliquity is still maintained, so as to assure the operator that the point is in the groove, and to ensure its non-escape therefrom. In this way the whole length of the groove is traversed, and the point of the blade finally arrested by the closed end of the staff. It is then best slightly to withdraw the blade and to complete the division of the soft parts as it is brought out, the knife being "lateralised" and made to cut in a semi-circular direction between the anus and the tuberosity to a point rather behind the level of the anus. This whole cut may measure from $1\frac{1}{8}$in. to $1\frac{1}{2}$in. according to the development of the parts. I was led to alter the construction of the rectangular staff. After trying various expedients, I have finally had the staff hinged by a very simple and effective mechanism, ably executed by Mr. Hilliard, of this city (see woodcuts illustrating the article), so that when being introduced it can be placed in the most favourable position for being passed along the canal, and when it is in place, by turning a screw in the handle, it is firmly fixed in a rectangular position. The pressure of the left forefinger in the rectum brings it to its right-angled position (and that it cannot pass), and then two turns of the screw fixes it there. By this simple plan much is gained. First, all difficulty of introducing it is overcome. Secondly, the heel of the staff (which is the point we seek for, and which there may be a difficulty in finding if the staff is a small one), may be greatly enlarged (widened and made more easily detected), and so more surely entered. And, thirdly, the removal of the staff from the urethra is also facilitated, as by reversing the screw the horizontal portion is allowed to fall, and so the angular shape of the staff done away with. (Prof. G. H. B. Macleod, p. 230).

Spermatocele.—Use the strong tincture of iodine of the Edinburgh Pharmacopœia. From the almost unvarying

success attending its use by Mr. Syme and Mr. Lister, it may be looked upon, if not as a certain cure, at any rate as greatly superior to the weaker preparations of the same drug. (Mr. J. Baker, p. 243.)

Stone in the Bladder.—*Removal by Litholysis or Solution.*—Dr. Duncan says that some cases of stone can be dissolved. He introduces an india-rubber pouch into the bladder to catch the stone. (See woodcut of instrument at p. 227.) The stone, being secured in the pouch, perfectly isolates it from the walls of the bladder, so that the next step in the operation is the introduction of the solvent, which is done by a graduated glass syringe, the nozzle of which fits into the end of one of the tubes. In theory, different solvents will be required for the solution of the different formations; but, practically, nitric acid, will, I think, be the solvent; I might say, *par excellos*, even the cholesterine formation is said to be soluble in it as well as in alcohol. No doubt, it will be at once asked, Will not the acid destroy the pouch and tubes, and in what strength will it be used? Experience has taught me that the rubber is not acted upon by the acid, and the strength will be according to the length of time wished for the operation, and the size of the stone, with the general state of the patient. A solution containing about fifty per cent. of acid disintegrates them very rapidly. The operator must be governed in the strength of his solution chiefly by the formation, of which the microscope must be his guide. I shall have to defer until another time some experiments concerning the different degrees of solubility of the different formation of vesical calculi. (Dr. G. C. Duncan, p. 226.)

Uræmic Convulsions.—There are two modes in which uræmic convulsions may be prevented, namely: first, by means directed towards removing the morbid quality of the blood; and, second, by remedies which lessen the reflex excitability of the nervous centre. It is a well-known fact that the inhalation of chloroform or ether-vapour invariably puts a stop to uræmic convulsions, and often wards off an attack after premonitory symptoms, such as convulsive twitchings of certain muscles, have occurred. It has sometimes been supposed that the anæsthetic acts by relaxing the cerebral arteries; but an observation of Kussmaul and Tenner points to a different explanation. These experimenters found that, if animals are etherised, no convulsions occur when they are bled to death, or when their intracranial circulation is arrested by ligatures. It appears, therefore, that the anæsthetic vapours prevent or stop convulsions, by lessening the reflex excitability of the nervous centre. The undoubted influence

of repeated full doses of bromide of potassium, in warding off uræmic convulsions, is probably to be explained by its soothing sedative influence on the nervous centres. The bromide is a very useful remedy for the painful muscular cramps which are of common occurrence in the advanced stages of all forms of renal degeneration. These cramps, which are especially frequent and severe in the lower extremities and during the night, are no doubt to be classed with the results of uræmic poisoning, and in not a few cases they are the precursors of more formidable nervous disorder. They may, in some cases, be entirely prevented by a draught containing twenty grains of bromide of potassium, with five grains of carbonate of ammonia at bed-time. (Dr. G. Johnson, p. 70.)

DISLOCATIONS AND DISEASES OF THE BONES AND JOINTS.

Abscesses Connected with Bone. — *Hyperdistension with Carbolised Water.*—Not only may an abscess of a simple character be advantageously treated by hyperdistension with carbolised water, but one connected with bone even may be so treated, as shown by the case of a chiid with psoas abscess from carious spine. Having been placed under the influence of chloroform, an incision was made in the most prominent part of the swelling, and of sufficient size to admit one's finger. From this opening a large quantity of thick and healthy-looking pus escaped, and when the flow was becoming slow the nozzle of a large syringe was introduced, and the cavity completely washed out with a warm solution of carbolic acid and water. The edges of the wound also having been held securely over the syringe, the cavity of the abscess was more than fully distended with the injection, which was allowed to be retained "while one with moderate haste might tell a hundred." A drainage tube having been introduced well up into the loins, and the wound covered up with oiled lint, the boy was sent back to bed. When I went to see him an hour afterwards, he was half sitting up in bed playing with his toys, and talking to other children in the ward. The abscess was syringed out every other day. The discharge continued sanious after the second injection, and the case went on most satisfactorily. The great feature in this method of treatment of psoas abscess is its simplicity, and the absence of all special and intricate dressings. All that are required being a scalpel, the warm solution of carbolic acid, of the strength of one part in thirty, an ordinary enema syringe, a long probe, and a drainage tube. The best dressing consists in fine picked oakum, for soaking up the

subsequent discharges, and a bandage. (Dr. Edmund Owen, p. 183.)

Angular Curvature of the Spine.—*Plaster of Paris Bandage.*—There are two principles of treatment which have at different times found favour. The one is directed to making the ulcer as small as possible, by allowing collapse of the vertebræ above and below; the other aims at keeping those bones asunder. The former of these, enjoining rest, chiefly by means of recumbency, obviates that part of the pressure which is caused by superincumbent weight; but the other part, that by muscular action, is ignored. One sees such patients worn with long lying, bending up more and more, till at last those who get through the treatment rise from their beds with the deformity as fully developed as the circumstances would permit. What a weary matter it is, too, this condemnation to a bed or couch for years, one not at all likely to promote that state of health which is needed for the cure of strumous ulcerations, as well as for the establishment of such sthenic actions as might promote anchylosis The other method, that which aims at minimising the deformity by keeping the bones apart, effects at the same time diminution of pressure, especially at the front edges of the vertebræ. For a great many, indeed for several hundred years, mechanisms for stretching the spine have been alternately vaunted and discarded. The idea is correct. Straightening the spine so as to annul pressure is the first step; but how to keep away pressure by some really effectual yet light appliance has been the difficulty, and this has been solved by Dr. L. Sayre. The plan is simple. The patient is to be suspended by the axillæ and head for a few minutes, while a plaster-of-Paris bandage is put on rapidly and allowed to harden. During this suspension, the weight of pelvis and lower limbs straightens to a certain degree the spine, and then the plaster, when the patient is allowed to stand, prevents the figure falling forward again; thus keeping the retreating angle open and obviating pressure on the front of the vertebræ. Mr. Barwell has improved this method as follows:—When all is ready, the apparatus found to fit comfortably, and assistants duly instructed, a leaden wire is laid on the spine, and an accurate pattern of its curve is taken. The height of the figure is also measured. Then the patient is suspended, and a flannel bandage rolled upon the figure from about the level of the trochanters to the armpits. A small pad should be placed over each anterior spine of the ilium, and if we are dealing with a young woman a layer or two of wadding should be placed between the mammæ and

also on the outer side of the glands. I also fold a napkin three or four times, and place it over the central parts of the abdomen. Over all these a prepared plaster-of-Paris bandage is applied, more plaster is rubbed in, and the whole allowed to become very nearly hard. The patient is then taken horizontally out of the sling and laid on a couch, the pads over the crista ilii and the napkin over the abdomen are withdrawn, and thus a space is left over each prominence so as to avoid pressure, and also, over the abdomen, room for respiration. In a few minutes more the plaster is quite hard, the patient may be placed on his feet, and the height of the figure is again measured. In my experience, it increases from three-quarters of an inch to two inches. The wire should be traced on paper, and the pattern dated and put aside for future comparison. The patient, if the bandage have been skilfully applied, feels much more comfortable, and is able to stand, even to walk, with much greater ease than before. He should, indeed, be allowed, even encouraged, to get about. (Mr. R. Barwell, p. 167.)

Boracic Acid Ointment.—Prof. Lister's boracic acid ointment as a dressing for wounds in general is preferable to either dry lint or other dry applications, and also to water-dressing. For wounds, when hemorrhage has been stopped or can be caused to cease by the application of light pressure, it is very useful; for, owing to the smooth waxy consistency of the ointment, the dressing does not at all adhere to the edges of the wound, nor to the clot between its margin. The dressing can, therefore, be removed and replaced as often as is advisable for the examination of the wound, without disturbing the healing process. Any discharge that forms can also easily escape between the layers of ointment and the skin around the wound. The ointment is thus preferable to dry lint, except in those cases where there is a great amount of oozing, when the dry lint and blood may act beneficially by forming an artificial scab. Water-dressing may be regarded as an inefficient mode of poulticing, and wounds that do not require poulticing can be better treated with the ointment than with water-dressing; for prolonged water-dressing generally irritates the skin round the wound, which becomes sodden and sore; while the skin remains comparatively healthy under the ointment. The smooth surface of the ointment is less liable to destroy by friction or otherwise damage the surface of the granulations than is the lint. The ointment never sticks to the surface of the wound, and no pain or injury is caused on removal of the dressing. Another great advantage is that, owing to the antiseptic quality of the boracic acid,

the dressing need only be removed every second or third day, unless the discharge be profuse. Thus time is saved with hospital patients. I have noticed that small lacerated wounds —for example, of the fingers—will keep perfectly sweet for twenty-four hours under the ointment even in tropical climates, and here they keep quite sweet for two days; whereas water-dressing generally requires to be reapplied every twenty-four hours. In applying the ointment, the dressing should extend far beyond the edges of the wound on to the surface of the surrounding skin, so as to interpose a considerable antiseptic interval between the margin of the wound and the limits of the dressing. This is the more important in proportion to the amount of discharge. (Mr. Arthur W. Bateman, British Medical Journal, Sept. 22, 1877.)

Dislocation of the Shoulder-Joint.—Reduction of a dislocated shoulder-joint by the usual method of the heel in the axilla, is greatly facilitated by the following simple plan. A diaper towel is rolled up so as to make a thick and soft quasi-rope, and this is put round the head of the humerus in the axilla, and the ends given to an assistant, who with it lifts, as it were, the head of the humerus out cf its position by pulling upwards and outwards, whilst the surgeon makes traction upon the wrist of the patient in the usual way. (See illustration at page 165.) (Dr. Montgomery E. Ward, p. 163.)

Extreme Rachitic Deformity of the Legs.—*Subcutaneous Osteotomy.*—In a case of extreme external curve of the lower third of the bones of the leg, so great indeed that the outer side of the ankle and leg touched the ground when the child tried to stand upright, it was decided to perform subcutaneous osteotomy. This was done on the first leg by means of a chisel and hammer. The result was successful, but the wound was long in healing. The tibia was then divided on the opposite side. I did not use the hammer and chisel, but employed a tenotomy knife and a short straight narrow-bladed saw. The division was effected with the greatest possible ease through a small puncture made with the tenotome, and when the division was complete, and the leg brought into a straight line, the punctured wound was closed with collodion, and the limb put up on a back splint. On this occasion the spray was dispensed with. On the third day I found the external wound firmly closed up, and all that we had to wait for was the consolidation of the divided bones. This proceeded satisfactorily, and, as you see, the limb is now quite straight and strong. In a word, it required six weeks to heal the wound made with the chisel, and two days to heal

that made with the tenotome and saw; so that it certainly seemed to me that we had in these two operations a fair example of how to do it, and of how not to do it. Such cases as these serve to support the principle that subcutaneous operations are free from risk. You here see many different structures, skin, fasciæ, muscles, vessels, nerves, and bones, divided without any disturbance of the general economy, and we are in a position indeed to affirm that subcutaneous surgery ought never to be followed by putrefactive inflammation. I do not say by inflammation, for this may happen in spite of every precaution, and, indeed, does happen. Perhaps the experiments of M. Guérin in 1840 demonstrate with more force than others the absolute safety of subcutaneous wounds. Amongst many other experiments on an equally liberal scale, he, for example, subcutaneously divided in a dog all the spinal muscles from neck to sacrum, and in the same victim, all the muscles, vessels, and nerves on the outer side of both thighs, and all this butchery was done without constitutional disturbance, and with rapid and completely non-inflammatory healing. The exclusion of air, then, secures a good result, and renders an operation safe; but we now know that it is not the air which does harm, but something in the air, and that if we can exclude this something we may allow the air free entrance. The researches of Pasteur, corroborated by the experiments of Tyndall, Lister, and Roberts, lead us to the conclusion that the cause of all putrefaction—and it is putrefaction which we have to dread—is the presence of bacteria, and hence to exclude bacteria, or what comes to the same thing, to destroy the bacteria already present, is to render putrefaction impossible. (Mr. S. Messenger Bradley, p. 176.)

Oakum as an Antiseptic Dressing.—Oakum is simply tarred rope reduced to its original state of flax or hemp, with the addition of the tar. For its use as an antiseptic dressing either of two methods may be adopted. In the first, the dressing is made as simple as possible: a single strip of lint soaked in carbolic or spirit lotion, applied next the wound, with a roll or two of bandage to hold it in position, and the stump then surrounded with oakum and another bandage if necessary. Or you may apply the oakum next the wound, and this method is less objectionable than would at first sight appear; and, for reasons to which I shall presently allude, the flax may be much preferable to cotton in contact with a granulating surface. The advantage of this dressing is, that for stumps, it is simple of application, it makes a soft pad on which to rest, and, what is of the greatest importance in the

wards of an hospital, *it effectually keeps down any offensive odour*—and more, its effect seems to be to destroy the putridity of the discharge, while the pleasant tarry smell is strongly suggestive of the sea, of ships, and of fresh tar. In most of the cases in which I have seen it applied, healing has taken place by the first intention, and this is, no doubt, partly accounted for by the freedom with which the serum is discharged from the wound, and by the prevention of ingress to any substance which would favour decomposition. (Dr. R. Esler, p. 155.)

Sawdust Pads (Surgeon-Major Porter's).—The sawdust is obtained by preference from the Memel pine; that from red deal may also be used, either of these containing a large amount of terebine. The dust from hard wood will not answer, as Mr. Porter finds that it does not absorb freely. It has first of all to be well sifted, for, as supplied from the works, it often contains coarse fragments which would cause, under pressure, hurt or inconvenience. The fine sawdust is then enclosed in muslin of such quality as will just prevent its escape. The bag, when made, is shaped for each case as may be required; when about three-fourths full it is closed, and is then quilted, otherwise the wood-dust will gravitate, or under pressure will be displaced entirely from certain parts of the bag. As to the muslin, I have ventured to depart from Mr. Porter's practice in using ordinary instead of antiseptic gauze, no advantage being gained by the use of the latter. The pads thus made are applied either to side splints, or to cover an ordinary back splint (as for a compound fracture of the leg), or over abscess wounds, or over suppurating surfaces, or over dying or dead tissues; they are used, in fact, either as pads or as the dressing over any part. I do not feel disposed to rely entirely upon these pads for keeping parts absolutely clean; but, in conjunction with carbolised oil, or with some kindred dressing, they are amongst the best pads with which I am acquainted, and I consider that we are much indebted to Mr. Porter for giving us an appliance which is simple, inexpensive, and efficacious. (Mr. G. W. Callender, p. 172.)

Ununited and Adherent Tendo-Achillis.—The tendo-Achillis of a man had been divided by an accidental blow from an axe, and the separated ends had not united after an interval of two months. The parts were cut down upon, and no trace of organised material was found connecting the widely separated ends. Having first freed the upper end from its attachment to the skin and cicatrix, I pared both ends of the tendon, removing a thicker slice from the lower

than from the upper one, on account of the rounding off of the former. Then, by flexing the leg to almost a right angle, the ends were brought in contact and secured by means of two prepared catgut sutures of double "medium" thickness. The limb was then firmly adjusted in this flexed position by applying the apparatus usually employed in the treatment of ruptured tendo-Achillis. The operation was performed under the antiseptic spray, and the wound was dressed in the usual way. The antiseptic dressing was continued, and changed as often as required, until April 1st, when, the wound being quite superficial, boracic lotion was substituted for it. Three weeks after the operation the parts were carefully examined, and, as good union had taken place between the ends of the tendon, the limb was slightly straightened, so as gradually to stretch the new material and obtain the proper lengthening of the tendon. (Mr. T. Annandale, p. 170.)

AFFECTIONS OF THE SKIN, &c.

ACNE.—Without any change in the patient's habits, you may often get an acne eruption so nearly well that he will regard it gratefully as a cure. The chief local measure consists in destroying, by means of a fluid caustic, the inflamed follicles. With a fine-pointed glass brush, or a bit of soft wood cut to a point, you touch the inflamed spots from day to day. Take great care not to apply too much. In the left hand should be a roll of blotting-paper with which to absorb the fluid if it has been deposited too abundantly. The best fluid to use is the acid nitrate of mercury. It will usually be necessary to repeat the touching once a week for a month or two, carefully seeking out every fresh spot. After that the patient should still see you once a month, in order that the cure may be kept up. The acid thus used does not leave larger scars than the spots would themselves do. (Mr. J. Hutchinson, p. 262.)

Acne Rosacea.—Chrysophanic Acid Ointment.—A lady, forty-five years of age, had an aggravated form of acne rosacea, the tubercles occupying almost the whole of the face. She was treated with chrysophanic acid ointment as an external application to the face, and with glycerine of nitrate of bismuth as an internal remedy. No other remedy, external or internal, was used from first to last. She commenced treatment Jan. 19, 1877. On Feb. 27, she presented herself quite free from any trace of her former eruption. I attribute the alteration she experienced purely to the action of the chrysophanic acid ointment. It consisted at the first of twenty grains of chrysophanic acid dissolved in an ounce of lard at the temperature of an oil-bath. For the last ten days

of the treatment, however, the strength of the ointment was raised to that of forty grains of chrysophanic acid to the ounce of lard. The ointment in either case was regularly, three times a day, rubbed well in all over the face, avoiding only the eyelids and the lips. From the beginning to the end the patient never experienced any smarting from this energetic treatment. However, occasionally the face became a little puffy, as if slightly swollen. Throughout this treatment the face became more or less stained by the action of the ointment, but it was not *very* much stained. The complexion of a field labourer about autumn time is often quite as dark as this patient's face was at any time of the treatment. The stain proved, of course, quite transient, passing away completely after a few days' discontinuance of the ointment. (Dr. Balmanno Squire, p. 263.)

Bronchocele.—*Hypodermic Injection of Ergotine.*—In a case of bronchocele of very large size the growth filled up the whole space between the chin and clavicles, being especially prominent below and on the right side. The tumour felt exceedingly hard and tense, and its weight caused additional distress to the patient. I determined to endeavour to reduce the tumour by injecting a solution of ergotine hypodermically. I used in the present case the ergotine discs of Messrs. Savory and Moore, which are most convenient, and thoroughly reliable. In all cases sixteen injections were made over the tumour, and as closely in contact with its substance as possible. I commenced with one disc equal to one-third of a grain, and increased the amount gradually to three discs, equal to one grain of ergotine. They were simply dissolved in distilled water, and injected at blood heat. The first four injections were made daily, the next four at intervals of two days, and the others at longer intervals, the whole extending over two months. The results were soon apparent, and were most satisfactory. The tension and dense consistence of the tumour first of all diminished rapidly, with great relief to the dyspnœa and dysphagia. The whole mass became gradually reduced in size, until the left lobe, which had always been the least, returned to its natural size, while the right and middle lobes certainly returned to half at least of their former dimensions. (Dr. J. G. Sinclair Coghill, p. 272.)

Large Carbuncle on Back of the Neck.—*Multiple Incisions.*—Make one incision about an inch and a-half or two inches long, as the case may be, over the centre of the carbuncle when the slough has formed, and from four to six counter-openings round the central one, varying in size from a quarter to half an inch; then plug the openings with strips

of lint steeped in some stimulating application, and lay a poultice over all, dressing the carbuncle in the same way every day until all the sloughs have separated. This plan has all the advantages of the crucial incision, without any of the disadvantages of that method. (Dr. Montgomery A. Ward, p. 255.)

ECZEMA, IMPETIGO, &c.—*Caoutchouc Coverings.*—M. Besnier attaches much importance in his practice to the employment of caoutchouc coverings in certain diseases of the skin, especially eczema and impetigo. In fact, treatment is so simplified by this means that it is almost impossible to speak too highly of it. According to the part affected, the form of the application is varied. For the scalp, a plain caoutchouc cap is employed; for the face, the vulcanised cloth or caoutchouc is cut, so as to make a mask fitting accurately the features, with holes for the eyes and mouth, and fixed behind the head by tapes. These masks should be carefully made, and the different pieces sewn together and arranged so as to insure close contact with the face. On the body or limbs, if the eczema do not occupy a large surface, a simple piece of vulcanised cloth, completely enveloping the diseased part, is fixed by bandages. If the diseased area be very extensive—*e.g.*, over the two lower extremities, or the trunk and arms, the patient must be provided with a pair of drawers or a waistcoat of caoutchouc, carefully applied and tied with strings. When thus used, the caoutchouc produces no irritation or other inconvenience. At first the perspiration which forms beneath it is very abundant; the parts are macerated and rendered pliant by the moist atmosphere in which they are maintained. After a little time the crusts are detached, and it frequently happens that a cure is obtained more quickly by this than by any other means. The only precaution to be observed is to wash thoroughly the caoutchouc dressing every day; and, if the sweating be extremely copious, it will be necessary to change the dressing in the course of the day. But in proportion as matters improve the sweating decreases, and the cure is known to be complete when no moisture is to be observed upon the caoutchouc after removal. (M. Besnier, Dublin Journal of Med. Science, Oct., p. 317.)

PSORIASIS.—Dr. Squire recommends the chrysophanic ointment (acidi chrysophanici ʒ ij., adipis ℥ j.) Dr. Whipham cured a patient of what on May 31st was a "copious eruption of psoriasis over the trunk, arms, and legs," by the exclusive use of chrysophanic acid ointment, with the following result; namely, that "on June 21st, exactly three weeks after the commencement of chrysophanic acid ointment," he found

that "with the exception of one or two spots, each rather less than the size of a pea, on each wrist, she was quite free from all trace of the skin-disease." On July 22nd he again "saw her, and found all traces of the eruption gone and her skin natural. She had discontinued the ointment for some weeks." He adds that "it was not without a feeling of despair that I had recourse to chrysophanic acid; the result, however, and the rapidity with which that result was brought about surprised me extremely, a surprise which is not lessened by the fact that the girl had suffered from the skin-disease for five years and a quarter at the time when the acid was first employed, and that she was entirely free from psoriasis in twenty-one days." Dr. Whipham's surprise was only natural. The efficacy of chrysophanic acid in psoriasis is certainly one of the most astonishing facts in modern therapeutics. I refer incidentally to this part of his paper because it was in this Journal that chrysophanic acid was first made known to the medical world as a remedy of the utmost efficacy in psoriasis, and, for another reason: because it fell to my good fortune to make that particular discovery. If you wish to use phosphorus you may get the French capsules called "perles," which contain each one-thirtieth of a grain. (Dr. Balmanno Squire, p. 245.)

Sebaceous Cysts.—The following treatment applies to sebaceous cysts on the scalp, which arise from the occlusion of the hair follicles. Their removal is very simple. The best plan, I think, is to transfix the cyst with a sharp knife, and to turn it out with a scoop, which should be carefully insinuated at the angle of the incision, and, and so used as to go gently round the circumference of the cyst, the operator bearing in mind the size and shape of the tumour. No force should be employed; for, simple as such operations appear, they are uot unfrequently followed by a good deal of inflammatory action, and occasionally by erysipelas. So much stress do I put on the importance of not using roughness, that I recommend in those cases in which adhesions exist, that a few light touches of the knife should be used, keeping quite close to the cyst, rather than the growth should be dragged away with violence. Immediately the cyst is enucleated, a pad of dry lint should be applied; and, as a rule, the wound heals by primary union. (Mr. F. Mason, p. 250.)

Small-Pox.—A mixture of glycerine, gelatine, carbolic acid, and water is very useful in painting the surface of the body; it quite prevents the odour of the disease, and entirely alleviates the troublesome itching, so much so that patients ask to have it by the bed-side so that they may paint themselves frequently. (Mr. E. Prideaux, p. 266.)

Salicylic Acid in Small Pox.—Regarding germs or minute bioplasmic particles as low forms of life, if we are able to introduce into the blood and tissues something that is fatal to their activity, we shall necessarily mitigate a disease depending in great measure upon that activity; it is necessary, then, that we should use a drug that will be absorbed into the blood without undergoing material change, and of which we may use considerable quantities without ill effect. It appeared to me that in salicylic acid we have a drug that fulfils all the required indications; it is a powerful antiseptic, and is very fatal to all the lowest forms of life; it passes into the blood materially unchanged, probably in the form of the neutral salts of soda and potash, and appears in the urine in the form of salicyluric acid, and may be detected by the addition of a few drops of tinct. ferri perchloridi, which produces a characteristic violet colour. When pure it may be given in large doses, as much as half an ounce in twenty-four hours; when given in these large doses it is apt to produce a marked depression, but this I find is obviated by giving small doses of carbonate of ammonia. The best mode of administering it I find to be in solution with carbonate of ammonia and bicarbonate of soda in the proportion of five grains of each of these to twenty grains of the acid. This mixture is most pleasant to the taste and perfectly unirritating to the intestinal canal, and in some sixty or seventy cases I have never known it to produce sickness. (Mr. E. Prideaux, p. 264.)

Tinea Tonsurans.—There is no doubt as to the fact that no agent with which we are acquainted is to be compared to carbolic acid for the destruction of organic life without destruction of organic matter, and that no agent is so useful in treating parasitic diseases of the skin, from the fact that, in proportion to its destructive action on the organisms which produce them, it is the least injurious to the cutaneous tissue. Attention to details is of such importance in the treatment of tinea tonsurans, that it is necessary to add to the directions given, the remark that the hair should be cut close with scissors, and that the oil should be rubbed into the skin for a few minutes. The treatment should be continued for at least a fortnight after the disease has apparently been cured. Either of the following prescriptions may be used. The first has the advantage of not becoming thick or dry from evaporation, while the second is cleaner and cheaper. 1. ℞. Sulphuris precipitati, zinci oxidi, āā ʒj; olei olivæ f. ℥j; acidi carbolici gr. xvj. 2. ℞. Sulphuris precipitati, zinci oxidi, āā ʒij; glycerini, aquæ, āā f. ʒiij; acidi carbolici gr. xvi. (Dr. R. J. Lee, p. 270).

AFFECTIONS OF THE EYE AND EAR.

Action of Atropia on the Eye.—For therapeutical purposes the sulphate of atropia is the most convenient form in which to use atropine. This salt is very soluble in water, requiring no addition of rectified spirit. If the usual four-grain solution of the Pharmacopœia be dropped into the eye, it will in most cases produce in half-an-hour complete dilatation of the pupil. It is then that the power of accommodation becomes impaired, and near objects cannot be distinctly seen. In about an hour later, *i.e.*, an hour and a half from the instillation, there is more or less complete paralysis of accommodation, and no objects within twenty feet can be distinctly seen. When complete paralysis of accommodation is once produced, it often happens that normal accommodation does not return for a week or a fortnight. By using a weak solution of atropine, it is very easy to cause mydriasis without paralysing the accommodation; hence, for purposes of ophthalmic examinations, it is wise to use a minimum quantity of atropine; though, for therapeutic purposes, it is usually of extreme importance that the accommodation should be paralysed. Let me now sum up in five sentences the wonderful effects of atropine upon the eye: it causes mydriasis; it lessens the blood-supply; it lessens intraocular pressure; it paralyses the accommodation; it causes local anæsthesia. Stramonium and hyoscyamus are almost identical in their action with belladonna; hyoscyamus is, however, believed to act more distinctly as a hypnotic; and most of us would use hyoscyamus for this purpose when we would not think of prescribing belladonna. (Mr. Talfourd Jones, p. 276.)

Action of Gelsemina on the Eye.—Gelsemina locally applied readily dilates the pupil, and, when used of sufficient strength, temporarily overcomes the accommodation. It is preferable to atropine in cases where the power of accommodation is not great, where it is necessary to overcome the accommodation for a short time only for the purpose of estimating the degree of ametropia, because its effects are more transient, and the confusion of vision during its action is less. To ensure paralysis of accommodation within three hours a solution of at least eight grains to the ounce must be used every fifteen minutes for the first hour, and every half-hour afterwards. (Mr. J. Tweedy, p. 283.)

Inflammations of the Conjunctiva and Cornea.—In the treatment of most of the commoner forms of disease of the conjunctiva, cornea, and iris, the most successful practitioner will be he who, in addition to making a wise use of the neurotic and constitutional remedies of which we have spoken,

also knows best how to apply local remedies. Let me briefly say that, where the conjunctiva is inflamed, metallic astringents are indicated, and that the best are nitrate of silver and acetate of lead. When the cornea is inflamed or ulcerated, there is no local remedy equal to atropine; and metallic astringents must be avoided, though, in certain stages and under certain conditions, a weak red precipitate ointment is often of service; and, when this is indicated, there is no preparation, to my mind, so convenient, so useful, and so handy as Cremer's pomade. (Dr. Talfourd Jones, p. 283.)

TINNITUS AURIUM.—*Hydrobromic Acid.*—Hydrobromic acid has the power of antagonising the ear symptoms occasioned by large doses of quinine. It is also efficacious in analogous states of the ear produced by other causes. The cases should be selected with a view to their appositeness to the presumed physiological action of the drug; and the indication which should be regarded as most distinctly pointing in this direction is that the noises have more or less of a pulsating, or, as the patient will describe it, a "knocking" character. The existence of vertigo, if present, will rather confirm the indication for the exhibition of the acid. The proper dose of the acid is fifteen minims every four hours. Two points appear important to secure the success of the drug: 1. The auditory apparatus must be clear of any well marked objective morbid process. 2. The tinnitus should present the characters of congested blood-supply, already alluded to. (Dr. E. Woakes, p. 366.)

VENEREAL DISEASES.

SYPHILIS.—With the exception of the mixed chancre, every form of syphilitic primary lesion as a rule propagates its kind; this is especially so in the case of the soft and the hard chancres, the dry papule and the chancrous erosion, though often the direct offspring of similar lesions, are not unfrequently transmitted from certain secondary affections; *e.g.*, condylomata. This circumstance, coupled with the fact that the soft chancre rarely infects the system, while the hard sores are generally followed by secondary symptoms, led observers to doubt the unity of the poison, some even maintaining that there was a distinct virus for each kind of sore, while most went so far as to say that there were certainly two distinct poisons; the one giving rise to the soft chancre, which was not a poison which invaded the system beyond the first inguinal gland, the other the parent of the various indurated sores which always infected the constitution at large. As knowledge ripened and syphilographers multiplied, this doctrine proved diffi-

cult to hold, if not untenable; cases occurred where sores pronounced soft chancres by competent judges were followed by constitutional symptoms; instances of the auto-inoculability of the hard sore were from time to time reported, and in spite of ingenious theories that in such cases as these we had a dual poison to deal with, grave doubts were thrown upon the validity of the doctrine of duality, doubts which became clearly manifest in the evidence given before the Admiralty Commission in 1870. Though "duality is dead," it yet speaks, and furnishes us with useful hints in both prognosis and treatment. It is the very general rule for the syphilitic primary sores to breed true; the soft sore produces the soft sore, and as the virus is wholly thrown off in pus, requires no mercurial treatment from first to last; the hard sore gives rise to the hard sore which infects the entire economy, and requires a lengthened mercurial treatment to remove its eradication from the system. (Mr. S. Messenger Bradley, p. 308.)

The soft sore is only exceptionally followed by secondary consequences, and it has therefore become the rule to abstain from giving mercury in such cases. This is, in my opinion, the greatest improvement in the treatment of the primary form of the disease which has been introduced in recent times. With respect to the treatment of the indurated primary sore modern opinion is divided. After-consequences may often be prevented altogether by resorting to mercury as soon as the primary induration becomes pronounced, and by persisting in it till it has completely passed away. If we are right in believing that the induration is an indication that the system is already contaminated, it must be right to attack it at the outset with the most efficient remedy we possess, and it is reasonable to expect that the disease may often be thus prevented from passing beyond its initial stage. All attempts to destroy the primary sore at an earlier period by cauterisation should be confined to non-indurated sores—in them it will often be successful, and can do no harm; but cauterisation and even free excision is useless after induration has been established. The induration will, after either method, as I have repeatedly seen, return before the wound heals, and the progress of the disease be in no way interrupted. Of the value of mercury in the treatment of secondary affections I need hardly speak. Its efficacy is now almost universally admitted in the removal of existing symptoms, in hastening the progress of the disease towards a favourable termination, and, where important structures are attacked, as in iritis, in the prompt arrest of morbid processes, which

without its aid would often be rapidly destructive. Mercury is pre-eminently the remedy for syphilis in the secondary stage, if used within proper limits, and with the precautions which fortunately are now very generally understood. When the secondary stage has been gone through, and when tertiary symptoms supervene, mercury, as a rule, is no longer beneficial. It was through its persistent use in this stage that it became from time to time discredited, and many were led to ascribe the occurrence of destructive ulceration and bone disease to its malignant influence. Fortunately, in the compounds of iodine a remedy has been found which is as efficacious in the tertiary stage as mercury is in the secondary. The beneficial effect of the compounds of iodine (iodide of potassium, sodium, or ammonium) seems to be confined almost entirely to the tertiary stage. I believe that in the earlier secondary period, at all events, it has no influence whatever. (Mr. J. R. Lane, p. 294.)

The induration of a chancre—supposed to depend upon its nature—really depends to a very great extent upon the tissue of the part upon which it is situated. A sore upon the prepuce is usually attended with much induration, whilst one near the urethral orifice is attended by little or none. This is owing to the abundance of cellular tissue in the former situation, and its comparative absence in the latter. There is another very important fact not generally known, that the further the sore is removed from the solid structures of the organ, that is, the glans and body, the less severe will be the consequent constitutional symptoms. Thus we find that marginal sores, which frequently occur along the extreme edge of the prepuce, although decidedly specific in their character, are seldom or never productive of extensive or severe "secondaries." A few patches of roseola on the body or some scattered lichen will probably be the only result. Not unfrequently even these are wanting, or perhaps a mild specific affection of the fauces will indicate that the poison has reached the blood. 2. Syphilitic sores affecting the internal or external prepuce may be considered more serious in proportion as their position is farther removed from the preputial margin, and nearer to the base of the glans and the main body of the penis. 3. Sores occurring on the body of the penis are more formidable than the two previous classes, and I have reason to believe that the nearer they are to the pubes the more may be apprehended. 4. Sores on the glans, more especially when situated at its base, involving the coronal groove and corresponding portion of the root of the prepuce, are most to be dreaded. They almost inevitably, even

in spite of early treatment, are followed by widely diffused and long-continued constitutional symptoms, frequently baffling the skill of the surgeon and wearing out the patience of the sufferer. 5. Sores contracted on the frenum become more or less important according to their extent. When they destroy the entire fold of the frenum, and involve its attachment to the glans and prepuce, they may be classed as sores of the coronal groove. (Mr. E. Cock, p. 291.)

MIDWIFERY, ETC.

Dysmenorrhœa.—*Liquor Ammoniæ Acetatis.*—Liquor ammoniæ acetatis is sometimes remarkably efficacious as a remedy in dysmenorrhœa. No satisfactory explanation of this fact can be given unless it acts by increasing secretion from the uterine surface. This seems probable, as, judging from the case related, it is most suitable when the pain precedes the flow and is relieved by it. One drachm should be given every hour with a little spirit of chloroform until relieved. (Dr. Clifford L. Drew, p. 339.)

Excessive Cauterisation of the Cervix Uteri.—What are the immediate pathological results of weekly or bi-weekly application of the nitrate of silver, which is the agent in most favour with practitioners, to the os tincæ? Inflammation and ulceration, lit up again and again by each application, followed in some instances by atrophy, and in others by hypertrophy of the cervix, with hard painful cicatrices and more or less contraction of the tissues and closure of the os, or even complete obliteration; endocervicitis and endometritis, the mucous lining of the cervical canal being more or less destroyed, and an ulcerated irregular surface remaining instead. Secondary functional derangements of the uterus follow, manifested first by dysmenorrhœa of a metrorrhagic character; and as the obstruction increases, regurgitation of the menstrual fluid takes place through the Fallopian tubes, setting up ovarian, perimetric, and general pelvic inflammations, with all their subsequent miseries. (Dr. J. Wallace, p. 318.)

Eczema of the External Genitals.—In middle-aged and elderly women eczema of the skin of the external genitals and neighbouring parts is not an uncommon disease, and it is most difficult of cure. Forbid the patient to take tea, coffee, or malt liquors, and let her substitute cocoa or milk for them. Let her take a compound colocynth pill with podophyllin occasionally at night. After a little preliminary treatment of this kind, paint the part well with collodion flexile, and then, when this is dry, apply a seeond coat. Let the patient paint the parts

twice a day in a similar manner. This plan of treatment may be expected to result in a cure. (Dr. H. Lawson, p. 267.)

Glass Specula.—Glass specula are easily washed, and, as they are transparent, one may see at a glance if they are perfectly clean; and this I consider a most important property, as by it the slightest particle of foreign matter may at once be detected. Secondly, the walls of the vagina may be seen through the glass, and fistulæ or other morbid conditions recognised; and in anteversion of the uterus, the cervix can easily be discovered and readily brought into the lumen of the speculum. And, finally, they give a very good light, are not acted upon by medicinal agents, and cost much less than the specula now in use. (Dr. James Murphy, p. 327.)

Prevention of Afterpains.—*Hypodermic Injection of Ergotin.*—Since the method advocated by Crede of squeezing out the afterbirth by external pressure has been generally adopted, there can be no objection to the employment of any safe and efficient mode of keeping the womb firmly contracted. Much may be done in this way by firm pressure by means of the hand of the accoucheur himself, and I am aware that it is the practice of distinguished obstetricians to administer a dose of some preparation of ergot by the mouth towards the close of labour. But the hand tires, and when anæsthetics are given, swallowing is impossible. In the solution of ergotin, introduced by Professor A. Simpson, we possess an agent available in every case; it is at once perfectly reliable, keeps well, and requires no preparation, while, if injected deeply over the glutæus. nothing unpleasant results from its use, save in some cases slight pain for a short time. In using ergotin there is one point to be attended to, arising out of the mode of action of ergot on the uterus. Although ergotin injected subcutaneously produces the physiological effect of the drug in the most rapid manner at present attainable, still it must be remembered that ergot acts gradually, its full influence is not manifested all at once. When given during labour the contractions brought on become by degrees more and more powerful and tetanic. So when ergotin is injected at the close of the second stage it does not at once bring the empty uterus after the expulsion of the secundines into that state of firm permanent contraction necessary to prevent after-pains. This must be done by the hand of the accoucheur himself, in the act of, and after squeezing out the placenta. The effect of the ergotin is now coming into play, and once the uterus is firmly contracted it will maintain it so. What I wish to insist on is that it will not do merely to inject ergotin and trust *to its unaided action*

from the first. We must be fully assured that the uterus is contracted before we leave it to nature and ergotin. (Dr. W. Allan Jamieson, p. 340.)

QUININE AS AN ECBOLIC.—A case is related in which a patient took ten grains of quinine in her eighth month of pregnancy. In less than an hour after most active labour pains came on, and before I could arrive she was delivered of a living male child, strong and healthy for his age. The membranes immediately followed. Strong uterine contractions continued for two hours, but in every other respect, she did well. (Dr. J. Patterson, p. 323.)

RIGID OS UTERI.—*Morphia.*—If upon examining our patient we find a rigid os uteri depending upon spasm, either direct or induced by reflex agency, if the pains are full, strong and regular, and yet no increase in the dilatation, but a hard constricting cervix with each access of parturient action, then we have a typical case calling for our remedy. If under these circumstances we give our patient from $\frac{1}{8}$ to $\frac{1}{3}$ of a grain of morphia, we shall frequently find that by the time that the morphia has been fully absorbed into the system the constricted os will feel more supple, will gradually relax, and the pains continuing unabated, labour will be completed, provided that no other impediment exists to delivery. (Dr. A. Wiglesworth, p. 316.)

RUPTURE OF THE MEMBRANES.—*When should the Membranes be Ruptured during Labour?*—It seems evident that *the function proper of the bag of waters should be limited to that of expansion only.* But the full dilatation of the os is effected, not by expansion alone, but also by longitudinal stretching. When, therefore, we find dilatation tardy from defect in degree or direction of the power alone, and not from any inherent character of the tissues, when once it is evident that the lower segment of the uterus is well expanded, the rupture of the membranes is the most effectual means of favouring the dilatation, by bringing the axial force into full action, and this irrespective of the degree of the size of the os. As the first stage of labour advances, the upper part of the vagina is dilated until it seems to coincide pretty closely with the upper part of the bony canal. When, therefore, a considerable portion of the lower segment of the uterus can be felt *in* the vagina, and not merely *through* its walls, expansion is certain to be complete, whatever may be the size of the parturient ring; and the tissues composing it are those of the cervix proper, and not the uterus. Under such circumstances the membranes may be ruptured with advantage. It is, however, unnecessary in many cases to wait for the full development of

the condition above described. I have taken the extreme state as being most readily understood, and indicating the direction in which our observations should be made. (Dr. William Stephenson, p. 343.)

Uterine Displacement.—*Cotton-Wool Tampons.*—The regular use of cotton-wool tampons, secures effective treatment through the following advantages:—1. The tampon forms a support sufficient to keep up the womb without exercising any undue pressure on the vagina, os uteri, bladder, or colon. 2. When dipped in an astringent solution, such as chlorate of potash, alum, tannin, perchloride of iron, diluted acetic acid, &c., it has a continued contracting effect on the relaxed parts. (Dr. Henry Lippert, Nice, p. 326.)

Uterine Hemorrhage.—In a case of violent uterine hemorrhage Mr. Grose found Bonjean's ergotine most effective. He says, "I bared the patient's arm and injected subcutaneously fifteen minims of the undermentioned solution of Bonjean's ergotine, which, with a hypodermic syringe, is part of the furniture of my obstetric bag. Within five minutes the womb had firmly contracted, and danger had passed. The solution I use is made by dissolving twenty-four grains of Bonjean's ergotine in one drachm of rose-water and glycerine, and five minims of this equals two grains, a usual dose. The ergotine itself is very like Liebig's meat extract in consistence and smell, and the solution in water is apt to spoil, but the mixture described is perfectly stable for a long period." (Mr. S. Grose, p. 337.)

Wasting Diseases of Children.—*Chloride of Calcium.*—Iodine and its salts occasion inconvenience from the development of their physiological symptoms, their proneness to cause dyspepsia and disturbance of the functions of the mucous surfaces generally; and their liability to induce, when long continued, emaciation, and also absorption, more especially of the osseous structures, greatly impair their otherwise valuable properties, and limit their employment, particularly in young persons. It is of course in the latter we are so often called upon to treat the various outcomings of the strumous diathesis, and it is precisely in this class of ailments that the chloride of calcium will be found to possess therapeutic properties, which render it altogether a much more potent, and certainly more manageable remedy than any of the various preparations of iodine. It is not only when glandular enlargement proclaims that the scrofulous cachexia has been developed, that chloride of calcium is of service. In those premonitory derangements of the digestive and assimilative processes, which in delicate and predisposed

constitutions so constantly precede or favour, if they do not indeed directly cause, the appearance of the more characteristic lesions of the strumous diathesis, the chloride of calcium is of the greatest use. In children and young persons when the sleep becomes restless and troubled, the breath fetid, the tongue foul and coated, the tonsils enlarged, the evacuations irregular and offensive, with deficient secretion of bile, I know no remedy approaching it in value. The colliquative diarrhœa, which so often accompanies this condition, and above all that obstinate lientery which is seen with hypertrophy of the mesenteric glands, yield to the solution of the chloride of calcium like a charm. In pronounced tabes mesenterica, the usual sequela to the condition above described when neglected, it is equally efficacious. I have also found this remedy of great use in controlling and modifying the characteristic alvine discharges in enteric fever. In fact turpentine stupes to the abdomen, and the solution of the chloride of calcium in milk internally, are what I have for long mainly trusted to in the treatment of the milder uncomplicated forms of typhoid. It is important to remember, that it should always be made of the crystallized or hydrated chloride of calcium, as the ordinary crude anhydrous salt gives a turbid appearance and an unpleasant caustic taste to the preparation. The old Edinburgh pharmacopœial solution will be found at present the most suitable, and it is certainly the most palatable. In the Edinburgh Pharmacopœia the Solutio calcis muriatis is composed of ℥viij. of the *crystals* of the salt in fluid ℥xij. of distilled water. The dose for an adult should vary from twenty or thirty to fifty minims in a wine-glassful of milk three times a day or oftener, according to the nature of the case, and should be taken, if possible, after food. When there is sickness or irritability of the stomach and bowels, the smaller dose more frequently repeated, and the vehicle warmed, will be found advantageous. Cod-liver oil is of course as valuable a companion to the chloride of calcium in appropriate cases, as it is to the iodides. Milk is in every respect the best, as indeed it should be, regarded as a cardinal element in the dietary of such cases as indicate the use of the chloride. (Dr. J. G. Sinclair Coghill, p. 362.)

Chloride of calcium possesses a most wonderful power in controlling, if not actually curing, many forms of tubercular disease. In my experience I have found no remedy on which so much reliance can be placed in tuberculosis as on this salt. In the wasting diseases of childhood, be these tubercular or not, chloride of calcium has proved itself to be, in my

experience, a therapeutic agent of inestimable value. The conditions which indicate the probable usefulness of the salt in children are, first of all, a falling off in flesh. The child may take his food heartily enough—nay, his appetite may be better than usual,—yet he becomes more attenuated every day, he is languid, ofttimes sleepless, and the pupils are always very much dilated—these symptoms being evidently due to a large amount of undigested food in the lower bowel. (Dr. Robert Bell, Lancet, Aug. 25.)

MISCELLANEA.

ALCOHOL.—*Physiological Action.*—Alcohol in small doses increases the secretion of the gastric juice and the movements of the stomach, thus aiding digestion. Although unnecessary in health, it is useful in exhaustion and debility; it increases the force and frequency of the pulse by acting reflexly through the nerves of the stomach. In large doses it impairs digestion by precipitating pepsin, and over-irritating the stomach, and may produce death reflexly by shock. After absorption in the blood it lessens the oxidizing power of the red blood corpuscles, this property rendering it useful in reducing temperature. When constantly or very frequently present in the blood, it causes accumulation of fat or fatty degeneration of organs; it undergoes combustion in the body, maintains or increases the body-weight, and prolongs life on an insufficient diet, being therefore entitled to be reckoned as a food; if large doses be taken, part of it is excreted unchanged. It dilates the blood-vessels, increases the force and frequency of the heart's action, imparts a feeling of comfort, and facilitates bodily or mental labour, not by giving additional strength, but by enabling a man to draw upon his reserve energy, thus giving assistance in a single effort, but not in prolonged exertion. The same is the case with the heart; but in disease alcohol frequently slows instead of quickening this organ, and by so doing economises instead of expending its reserve energy. By dilating the vessels of the skin alcohol warms the surface at the expense of the internal organs; therefore it is injurious when taken during exposure to cold, but beneficial when taken after the exposure is over, as it tends to prevent congestion of internal organs. (Dr. T. Lauder Brunton, Edinburgh Medical Journal Aug.)

ANÆSTHETICS.—*Bichloride of Methylene.*—All the advantages of complete anæsthesia, with fewer drawbacks, can be obtained by the use of bichloride of methylene or chloromethyl than by any other known anæsthetic. Given properly diluted

with air, the vapour of chloromethyl has, in my experience of ten years with more than 1000 operations of a nature unusually severe as tests of an anæsthetic, proved to be without a single exception applicable to every patient, perfectly certain to produce complete anæsthesia, relieving the surgeon from all alarm or even anxiety ; and its use has never been followed by any dangerous symptom which could be fairly attributed to it. By Junker's apparatus, air charged with methylene vapour is given—not the vapour itself—and, so employed, it has always been in my experience both efficient and safe. (Mr. T. Spencer Wells, p. 364.)

Etherisation and its Dangers.—When etherisation produces death it is partly caused by enfeebling the action of the heart, but mainly by arresting the movements of respiration, the pulsation of the heart continuing for a long time after all respiratory movements have ceased. The breathing is the key-note to the condition of the patient during etherisation, and every breath the patient draws ought to be carefully watched by the person who administers the ether. Nor is it sufficient to know that the patient breathes, for if the act is accomplished in a superficial or irregular manner, after the subject is fully under the influence of the anæsthetic, it is a symptom of danger not to be disregarded. The looks of the patient are another valuable indication of his safety when etherised. If the face becomes very dusky-red, as is often seen in the beginning of etherisation, when the patient does not get sufficient atmospheric air ; or very pale, as when the patient has been for a long time under its influence, or when syncope intervenes, with the look that patients have in a condition of collapse, with lips pale, clammy moisture upon the skin, and shrunken and contracted appearance of the countenance, together with shallow breathing, there is great danger. When the first symptoms of arrest of respiration are manifested, immediately discontinue the etherisation. The only rational treatment of this condition, and one that in most cases is successful, is artificial respiration. If we can keep the patient alive by this means until the volatile poison is exhaled, the returning sensibility will enable us to resort to other expedients. Artificial respiration should be immediately resorted to, and continued persistently. (Dr. Octavius B. Shreve, Massachusetts, U.S.A., Practitioner, Aug.)

Threatened Death from Chloroform.—Nitrite of Amyl.—Chloroform was being administered prior to removal of a fatty tumour from the loin of a woman of middle age, when suddenly the pulse flickered and stopped altogether, and to all appearance the patient was dead. The chloroform being

immediately withdrawn, some cold water was dashed on her face and her tongue pulled forward, whilst he commenced artificial respiration, but without success. Some nitrite of amyl was poured on lint, and held to her nostrils. In such emergencies it is impossible to judge the flight of time correctly; but he thought in ten seconds there was a flushing of the face, the pulse was again felt, and the all-important function of respiration was again restored; the woman being rescued apparently from the very jaws of death. (Correspondent of British Medical Journal, Aug. 18.)

GLYCERINE.—*Therapeutic Properties.*—Glycerine as a food in small doses increases the weight as it lessens waste of tissue, in consequence of its being oxidized in the lungs in preference to the fat of the body. Even the nitrogenous substances are more slowly consumed, as is shown by the diminished quantity of urea excreted in the twenty-four hours. Glycerine is a stimulant to the digestive functions, well tolerated, quickly digested, and absorbed so completely that unless taken in large quantities hardly any is found in the blood or urine. Elimination by the kidney begins within an hour of the time it is taken. It produces neither glycosuria nor albuminuria, and it has a laxative tendency. In large doses, or if taken suddenly into the stomach, it causes symptoms somewhat like those of acute alcoholism, but if taken gradually it only raises the temperature a little. The proper dose ranges from half an ounce to an ounce a day, and in many cases, M. Chatillon would recommend it as a substitute for cod-liver oil. It possesses the pharmaceutical property of preventing iron from combining chemically with cinchona, so that the three can be given in a mixture. (M. Chatillon, Dublin Jour. of Med. Science, June.)

HYPODERMIC INJECTIONS OF MORPHIA.—In regard to solutions of morphia for hypodermic injection, we have the *injectio morphiæ hypodermica*, which contains half a grain of the acetate of morphia in six minims. It is a fact that this is by far the best preparation to use for hypodermic injection, for, as a rule, two minims is sufficient to relieve pain and induce sleep in even the most violent cases of rheumatism, neuralgia, delirium, and cramp. Steel needles are the best. They can be made sharper than either silver or gold, and are thus more easily introduced. To every one who uses hypodermic injections of morphia frequently, it must be evident that the needles are very troublesome to keep in a pervious condition, as the solutions, especially that of the B.P., are apt to crystallize inside the tube. To rectify this, the syringe-case should contain a small phial of oil, as some of them are now constructed,

so as to afford an opportunity, after every injection, of passing the wires oiled. By this means the needles may generally be kept perfectly open, but if by any chance they do get blocked up, a drop of chloroform will be sufficient, if introduced by means of the wire, to remove the crystal. Another point in the construction of the syringe, where reform might be instituted, is the "washer" which keeps the needle tight when applied. From the constant dipping in the injection this often becomes softened, necessitating the application of a new one at frequent intervals, and eventually this softening permits the fluid to escape before entering the needle. I should recommend that the "washer" should be constructed of vulcanite, so as to stand a great deal of tear and wear. (Mr. John A. Erskine Stuart, Edin. Med. Journ., July.)

Tea and Coffee.—In all ailments, when I can get a patient to follow my advice, I prefer as stimulants—non-medicinal—tea and coffee *made with boiling-milk*, instead of with water, a plan I invariably follow also with infants and children with most pronounced good effect. (Dr. G. de Gorrequer Griffith, p. 331.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—ON CONTAGION.

(From the British and Foreign Medico-Chirurgical Review.)

The two problems at present absorbing the attention of scientists (medical and lay) are, the Evolution of Life, and the Germ-nature of Contagium. In the following pages we desire to side neither with the panspermists nor with the heterogenists. We shall trace the origin of contagium no further than published researches enable us, shall avoid all hypotheses respecting its relations to the evolution of life, and shall keep ourselves uninfluenced by any theory of fermentation and putrefaction. There exist a sufficient number and variety of observations to allow us to state certain potent reasons for applying the term *germ* to the ultimate entity of contagium, in so far, at least, as found in some contagious diseases. With the primary origin of such an entity or entities we do not concern ourselves, as this is as yet an unfathomed ocean; on the other hand, recent observations throw light, to a certain extent, on the behaviour of contagium under certain conditions; but of the further development of any one contagium, or its transmutation in form or substance, we know nothing positive. So, also, the relationship of contagia to one another is as yet a strange inquiry.

What reasons are there for believing that "many diseases are due to the presence and propagation in the system of minute organisms, having no part or share in its normal economy?" On what observations is the Germ Theory of disease based? What proof have we of the "*competence of germs to produce the phenomena of disease?*" In the next place, are all germs capable of inducing disease of similar natures—are they of like form, of uniform habits, or in what respects do they differ? Most investigations into the nature of contagium have been made with vaccine virus. To discover this contagium the microscope was first used, and Dr. Lionel Beale, in December, 1863, announced the discovery of transparent particles of extreme minuteness in vaccine lymph, and expressed his conviction that the contagious or active properties of the lymph lay in these particles. This observation is now admitted beyond doubt, but the relation of these particles

to infection, and their physical properties, were debateable points, till a few years later M. Chauveau submitted vaccine lymph to physical tests. Having proven, through separation by subsidence and through filtration, that the leucocytes and other constituents of vaccine lymph did not retain its infective power, but on the contrary, that its serum (which by microscopic examination revealed these minute sparkling particles) was still virulent; M. Chauveau proceeded to separate these "elementary granules" of the serum, now often termed micrococci, by means of diffusion. From such experiments he concluded "that the vaccinal serosity is not virulent, and that the activity of vaccine resides in the solid granules, either in all or only in one part of these little elementary organisms." M. Chauveau's researches, contained in two papers in the 'Comptes Rendus des Séances de l'Académie des Sciences' for 1868, led Dr. Burdon Sanderson to test these observations, improving the method of procedure in various particulars. Still later, Dr. Braidwood and Mr. Vacher employed diffusion for the purpose of separating the contagium of vaccine, and by attention to certain details overlooked by their predecessors, they may be said to have avoided all possible sources of error. All these observers have arrived at the same conclusion, viz., that "we possess the strongest proof (we can have no direct proof till we have learned how to wash off all trace of plasma from the bodies it suspends) that the contagium of the virus with which we are most familiar consists of transparent particles, not exceeding (according to Sanderson) the 20,000th of an inch in diameter, neither soluble in water nor in watery liquid, and not capable, without losing its properties, of assuming the form of vapour. Further, inasmuch as these particles do tend to subside, though (according to Chauveau) they never dispose themselves completely in the lower layers, we are assured that they are of a specific gravity only slightly greater than that of the plasma surrounding them, while, from their not sensibly diminishing the transparency of the liquid containing them, they refract light in the same degree as this, and by examining fresh vaccine microscopically any one can observe these minute, highly refractive, colourless bodies, to be moving, and to form groups of threes and fours. Now, these minute, distinctive particles (Sanderson's micrococci) are to be seen, not only in vaccine lymph, but also in the lymph of human and ovine variola. Hence it may be concluded that such are the physical characters of the contagium of the virus of all the diseases composing the variolous groups.

It may be concluded beyond dispute that certain infectious diseases are induced in the system by a "morbific agent, which is propagated in and given off from the bodies of the sick, and

is capable, when received into a susceptible healthy body, of producing in that body a disease similar to the one during whose course it was formed." (Maclagan, p. 5.) Further, the particulate nature of the contagium, or morbific agent of infectious diseases, is not only sufficiently defined by the observations above described, but it offers a ready explanation of a well-known, and on any other view, an inexplicable fact in the history of infectious diseases, viz., that of two persons situated in exactly the same circumstances, and exposed in exactly the same degree, to a given infectious disease, the one may be seized after a single exposure, while the other may be equally exposed for months without being attacked, and may even escape altogether. The contagium particles in a patient's breath resemble an enemy's bullets. The breath would be harmless without the particles, just as an enemy's powder would be without his bullets.

But what evidence is there in favour of regarding the contagium particles as organic germs or products of animal or vegetable development. As Dr. Baxter states with emphasis in his "Report on an Experimental Study of certain Disinfectants" ('Reports of the Medical Officer of the Privy Council,' new series, No. vi.), the "characteristic quality of a contagium or virus of a communicable disease is its capability of undergoing almost unlimited multiplication when introduced into an appropriate medium;" hence also its effects are to a great extent independent of the quantity introduced into the system. "This power of development, and this faculty of breeding true, were the first, and have ever been regarded as the chief arguments in favour of the germ theory of disease," observes Dr. Maclagan, "and it is difficult to see how these properties of contagium can be accounted for on any other view of its nature." It would be superfluous here to follow Dr. Maclagan's description of the chemical analogy of contagium to the process of crystallisation advocated by some. Another argument in favour of the germ nature of contagium is that, like all organised matter, it is prone to undergo change when separated from the conditions essential to its vitality; and this very proneness becomes an argument, not against, but in favour of the view which regards it as consisting of minute organisms. "If all the ova of every parasite came to maturity, few animals would be free from these pests. If every small-pox germ which came into existence developed and reproduced its kind in the same way as the parent from which it sprung, the civilised world would have been all but depopulated before the days of Jenner." While chemistry and physic, moreover, throw no light on the mode of growth or reproduction of contagium particles, the microscope has assisted though it has not

unravelled this mystery. Dr. Beale, for example, has figured the reproduction of these particles from the contagium particles found in the secretion in various diseases. He summarises the results of his observations thus in his brochure 'On Disease Germs,' p. 244. "Without, therefore, pretending to identify the actual particles of the living bioplasm of every contagious disease, or to be able to distinguish it positively from other forms of bioplasm, healthy and morbid, present in the fluids, on the different free surfaces, and in the tissues in such vast numbers, I think the facts and arguments I have advanced prove—first, that the contagious virus is living and growing matter; secondly, that the particles are not directly descended from any form of germinal matter or bioplasm of the organism of the infected animal, but that they have resulted from the multiplication of particles introduced from without; thirdly, that it is capable of growing and multiplying in the blood; fourthly, that the particles are so minute that they readily pass through the walls of the capillaries, and multiply freely in the interstices between the tissue elements or epithelial cells; and lastly, that these particles are capable of living under many different conditions—that they live and grow at the expense of various tissue-elements, and retain their vitality, although the germinal matter of the normal textures, after growing and multiplying to a great extent, has ceased to exist."

Still, more distinct evidence is furnished by Drs. Braidwood and Vacher's observations. They state in their 'First Contribution to the Life History of Contagium,' that the corpuscles, whose multiplication forms one of the characteristic appearances produced by the vaccine virus, "are to be seen in the crypts or hair-follicles budding or throwing off minute, round, highly refractive bodies." Similar corpuscles, seen in sections of skin affected by variola, are stated by these observers to be noticed germinating or throwing off spores on the fourth day of the eruption. At this point we exhaust our positive knowledge of the origin and reproduction of contagium. As stated by Dr. Sanderson many years ago, "all microzymes are not contagia, but all contagia may be microzymes." All forms and varieties of bacteria seen in contagious fluids and in diseased tissues are not contagium particles; and this is the point which specially concerns us.

"From the presence of bacteria in a given fluid we can postulate nothing as to its disease-producing properties" (remarks Dr. Maclagan). "The fluid may be full of bacteria, and yet be incapable of causing disturbance; it may contain few, or none of them, and yet a single drop introduced into the circulation may give rise to a most virulent disease."

The next stage in this inquiry refers to the circumstances

favourable to the propagation of contagium particles, and to those inimical to their reproduction. For the development of organisms certain external conditions are necessary; they will not grow in very low temperatures, and are destroyed in very high; they cannot thrive without water, and largely appropriate nitrogen in their growth. But contagium particles require something (as yet unascertainable) additional to these. "We know," observes Dr. Maclagan, "that the contagium of typhus, besides requiring for its development the conditions requisite to the propagation of organisms, has need of something more, which it finds in the human body; that the contagium of measles also requires its own peculiar element, which is also found in the human body, and is quite different from the element appropriated by the typhus contagium; and so on, with all the other contagia."

After the introduction into the animal economy of a poison or of a contagium a definite sequence of phenomena is noticed. Each has a definite and specific action; a certain time elapses after the reception of the substance into the system before morbid symptoms are developed; the severity of the symptoms varies with the dose of the substance introduced; the effects are modified by temperament or constitutional peculiarity on the part of the recipient; and after a time the substance is eliminated from the system. But, the existence of so many definite and distinct contagious diseases proves that the poisons which give rise to them are specifically distinct.

The view most generally accepted at the present time to explain febrile increase of temperature is that of Professor Virchow, which attributes this phenomenon to increased tissue change. According to this theory, "fever consists essentially in elevation of temperature, which must arise from an increased consumption of tissue, and appears to have its immediate cause in alterations of the nervous system;" but, as Dr. Maclagan remarks, this theory "serves to indicate rather than to explain" this febrile condition. "Why is there increased consumption of tissue? and why should the nervous system exercise other than its usual and normal action?" The reply to these queries would carry us a step further back than the point from which Virchow starts, but even then a constant and essential feature of the febrile state, viz. increased consumption of water would remain unexplained. The late Dr. Parkes, who confirmed and ably elaborated Virchow's theory, endeavoured to account for this last febrile phenomenon by supposing the existence in the blood of some substance which has an unusual attraction for water. He conjectured that this substance "may be some gelatinous compound which is formed in the rapid metamorphosis of the albuminous tissues, and which is ultimately

converted into urea and uric acid;" but, as argued in the monograph under review, "why is this gelatinous compound formed in such enormous quantity as it must be to cause so great a consumption of water? and what becomes of the urea and uric acid into which it is ultimately resolved?"

To give a satisfactory reply to such queries as these, which crop up the more closely we study the subject, it is necessary to abandon Virchow's view and seek for an explanation elsewhere. The late Professor Traube, in 1863, ascribed febrile heat, not to increased production, but to increased retention of heat, consequent on contraction of the minute arteries, an hypothesis the inaccuracy of which has been demonstrated since then by Liebermeister and Leyden. Later (1873), Senator has stated "that there are periodic diminutions of loss of heat, together with a constant though not great increase of heat production." The former he attributes to occasional contraction of the minute vessels resulting from the action of the fever-cause, the latter he attributes to increased oxidation. Thus we find one further problem to embarrass the inquiry. As Dr. B. Sanderson remarks (A, Appendix, in the 'Reports of the Medical Officer of the Privy Council,' No. vi. 1875), to explain the nature of fever and its relation to the febrile process two possibilities are open to us, "One is, that fever originates in disorder of the nervous centres, that by means of the influence of the systemic functions the liberation of heat at the surface of the body is controlled or restrained, so that by retention the temperature rises, and finally, that the increased temperature so produced acts on the living substance of the body so as to disorder its nutrition. The other alternative is, that fever originates in the living tissues, that it is from first to last a disorder of protoplasm, and that all the systemic disturbances are secondary. By both hypotheses it is tacitly assumed that fever is the product of a material fever-producing cause contained in the blood or tissue-juice, the morbific action of which on the organism is antecedent to all functional disturbances whatever." This leads us to the same conclusion as that arrived at by Dr. Maclagan, that fever "is a collection of different and various phenomena, all of which are abnormal, and all of which are developed subsequently to the reception of the contagium, and the co-existence of which in the body is conveniently characterised by the term fever."

The contagium being, then, the cause of the fever, becomes the cause of the individual phenomena which constitute the febrile state. To understand our position, it must be borne in mind that each of the minute organisms—contagium particles—reproduces myriads of similar organisms, and in so doing appropriates for their growth elements requisite for the nutri-

tion of the body in which they grow. Here we have a starting-point, from which, by careful logical reasoning, the various phenomena of the febrile condition are gradually evolved by Dr. Maclagan. He shows clearly that the nitrogen essential for the vitality of the contagium particles is derived from the constructive store, the albumen of the tissues, "that this consumption of nitrogen by the contagium particles is the primary cause of the rapid wasting of the nitrogenous tissues which takes place during fever," that the agency which thus eats up the tissues acts also in diminishing their nutritive supply. "Blood continues," he remarks, "to be supplied to and to circulate through the tissues, but it is blood charged with an organism which utilises for its own ends the materials which ought to go to nourish and build up the body." Moreover, not only do the contagium particles use up an excessive amount of nitrogen in their growth and deteriorate the blood, but they also consume water in large quantity, thus deranging nutrition and diminishing the bulk of the tissues.

Further and very valuable evidence in favour of this explanation of the most important phenomenon characterising fever Dr. Maclagan derives from Salkowski's observations on the elimination of potass and soda during fever. Salkowski found "that the quantity of potass eliminated on a febrile day was three or four times, sometimes as much as seven times, greater than on a non-febrile day;" and that "the soda discharge was reduced to a minimum during the febrile state, and rose again when the fever had ceased." Now, potass exists in largest quantity in the muscles and blood-corpuscles, while the chief seat of soda is the liquor sanguinis. The contagium particles, then, according to the experimental evidence quoted, consume the nitrogen, water, and potassium salts of the constructive store albumen found in muscle and blood; and this affords a full explanation of the wasting and irregular elimination of urea which characterise the febrile state. According to Dr. Burdon-Sanderson (ibid. cit.) clinical observations show—"That in the early stage of fever a patient excretes about three times as much urea as he would do on the same diet if he were in health; the difference between the fevered and the healthy body consisting chiefly in this, that whereas the former discharges a quantity of nitrogen equal to that taken in, the latter wastes the store of nitrogen contained in its own tissues."—*Medico-Chirurgical Review, October* 1877, *p.* 391.

2.—THE GLANDULAR ORIGIN OF DISEASE.

By Dr. B. W. Richardson, F.R.S., &c.

Dr. Richardson commenced by stating that the doctrine of a glandular origin of disease had occupied his attention for some

years, and that his views were based on a series of experimental researches on the mode of production and communication of these epidemic contagious diseases, anciently called "pestilences," and now known as "communicable," or "spreading diseases."

He then proceeded to explain at length the discoveries he had made to show that the fluids secreted during various stages of some communicable diseases were capable of propagating disease, and it was proved, he said, that a definite fever could be produced in an inferior animal by the fluid secreted from a patient suffering with that fever. He classed the disease produced by organic poisons as septinous instead of zymotic, he preferring the word septine from this poison. The diseases thus named are small-pox, measles, scarlet fever, diphtheria, typhus fever, typhoid fever, erysipelas, hospital fever, puerperal fever (or the fever which occurs to women in childbed), cholera, yellow fever, ague, glanders, boil and carbuncle, infectious ophthalmia. He explained the nature of these organic poisons, the mode in which they escaped from the patient by secretions, and how these poisons might be destroyed. He then touched upon the "so-called germ theory of disease." This hypothesis, for it was false to designate it as a "theory," was very old indeed. There is nothing whatever in fact in the clinical history of plagues that connects them with the hypothesis of an origin from germs produced without the body and entering it to fertilise it and create a decomposition. When I say there is nothing, I mean there is nothing except the analogy of which I have spoken above, and even that breaks down, for the analogy of a fertilisation of a field by seed means always a definite process of fructification and of results from it: whereas in the history of epidemic plagues there is no such definition. The germ hypothesis fails, however, on other grounds than the clinical. If it were true that living germs possessing an independent growth and vitality enter the animal body, that every disease of a communicable kind is due to its own external living germ, and that the germs continue to multiply and increase by an independent action of their own—if this be indeed true, why do the germs after a certain time cease to multiply and allow the sick person to recover? Why do they not go on multiplying until the person is infected in every part and fatally stricken? Who would get well from a disease due to living self-propagating contagions? Again, who, if the hypothesis were true, would escape fertilisation? A general fertilising diffusion of self-propagating matter in minute, invisible form, entering the body as air may enter, could hardly be expected to select a small minority of a population, and if it did so at the first, why should it do so when it had seized upon

many centres in which it could increase? But the history of all the communicable diseases shows that each epidemic affects individuals individually at different periods in the course of the epidemic according, as a rule, to the exposure to the infected, and that the period of the disease is limited by a development and a course rendered in certain periods of time. I need hardly add in objecting to this germ hypothesis, because the fact is admitted on all sides, that not only has no one ever seen a germ disease, but that no one has ever traced an order of germination in relation to any of the communicable diseases. When a really living self-propagating thing goes through its phases of life and action, like, for example, the yeast growth, we can trace it through its cause, and during its action on organic substances, can study its effects, the changes it produces, and the products of such changes. In the epidemic diseases we have no such guidance, no trace of it. These phenomena, indeed, are opposed to the idea of self-action of foreign vital material within the affected forms of pestilent disease. He then turned to a review of what he called the "glandular theory" of the origin of contagious diseases, and of advances he had made in support of that theory during the last ten years. In that time he had seen no reason to change his views on the subject of the glandular origin of communicable diseases. On the contrary every new observation has tended to confirm it, and to make the demonstration of the truth more definite. In continuance of observations he had noted that the number of the distinctly communicable diseases is closely related with the number of secretions. The poison of hydrophobia is from the salivary secretion; of diphtheria, from the mucous glands of the throat; of scarlet fever, he believed, from the lymphatic glandular secretion; of glanders, from the mucous secretion of the nasal surface; of typhoid, from the mucous glands of the intestinal surface; and so on. In some instances the blood itself is infected, and the corpuscular matter becomes the seat of the catalytic change. A second point that had occurred to him is that the matter or particle which sets up the poisonous action, instead of being living matter, is matter actually dead, and that its effect for evil depends, in fact, upon its being dead. He meant that dead particles of organic matter in contact with living, is the cause of the physical change which transforms the new particles of secretion into poisonous particles as they are brought up to the infected surface to be influenced by the infection. On the ground that the poisons were always of glandular origin, he had been led to the conclusion that under certain influences affecting glandular action the poisons may be made to originate directly through nervous impression without the necessary intervention of an infecting particle. In many

epidemics it is common to see a number of examples of the prevailing disease, the origin of which is traceable only to fear or anxiety. We call these nervous cases, and we try to define them as such and as distinct from cases due to contagion of a direct kind. But the symptoms are the same as those which follow actual contagion, and in epidemics of cholera they take even a fatal character. My theory, however, explains fully the reason of this. It indicates that an extreme nervous impression made on the glandular nervous supply, paralyses the glandular function, and thereupon produces the same phenomena as is produced in other instances by the action of a specific poison. The theory in this manner accounts for the origin of an epidemic disease from an impression made on the nervous system without the direct contact of poisonous matter, as well as for the after propagation of the disease by distribution of poisonous particles when that is communicated from an infected to a healthy person. It accounts equally for the production of disease, and of a poisonous glandular product under conditions of starvation and cold, by which the nervous tension is reduced. Again, it accounts for the production of disease, and of a poisonous glandular secretion under special atmospherical conditions in which the activity of the atmospheric oxygen is reduced in sustaining power. . . . The study of the glandular theory of the communicable disease has suggested to me another thought which observation of the diseases fully confirms—namely, that these diseases, like all which have their root in nervous derangement, present a distinct "heredity." The impression of disease made on a nervous centre is transmitted. There can be no doubt as to transmission of tendency to particular communicable diseases. Any physician in full practice can find any amount of evidence on the fact by simple natural inquiry. Typhoid fever is clearly a disease possessing hereditary transmissible quality. Diphtheria is the same, scarlet fever is the same, and small-pox, I should suspect, was once almost universally so characterised. These facts alone, one of them alone is sufficient to stamp the origin of the communicable diseases as from the animal body itself. It is certainly one of the best proofs of the truth of the theory of the glandular origin of the poisons. It will be seen at once by those who look with sufficient patience, that the mode of connection of diseases in hereditary line is the same as that which connects hereditary qualities of every kind, physical type, mental type, all else that binds many individualities into one family. Lastly, the study of the glandular theory of the communicable diseases enables me to offer the most rational explanation of the phenomenon of non-recurrence of the disease after they have once attacked a person susceptible to them. It is well understood that, as a rule, a person who

has been affected by a communicable disease is not affected a second time. To this rule there are many exceptions, but on the whole it holds good. On my theory the reason of the phenomenon is simple enough. They who are susceptible are born with a nervous impression tending to the production of a glandular secretion easily changed into poisonous secretion under the direct action of contact with poisonous matter, or even under the influence of a central nervous depression, whereby the glandular function is deranged. But when such a person has passed through the ordeal the tendency, for a time at least, disappears, owing to the complete modification of glandular function that has been induced, to the free elimination that has been established, and probably to the change in the nervous matter itself that has resulted from organic modification. Hence the organism becomes susceptible for a time, and if the tendency be not intense that time may mean the whole of life. Indeed, as life advances, and nervous susceptibilities, derived directly from ancestry, lapse into individual self-sustained susceptibilities, these tendencies to disease subside as a general fact, and lose their activity, if not their existence.—*Medical Press and Circular, Oct.* 10, 1877, *p.* 293.

3.—THE GERM THEORY OF DISEASE.

[The Editor of the Medical Press and Circular makes the following valuable comments on Dr. Richardson's lecture, an abstract of which is given in the preceding article. He says :]

The germ hypothesis of disease has met with a stalwart opponent in the person of Dr. Richardson, who last week delivered a lecture chiefly bearing on this question at the meeting of the Sanitary Institute of Great Britain in Leamington. It is well known that Dr. Richardson is no believer in a theory elaborated in, and promulgated from, the laboratories of two distinguished men, a chemist and a physicist, and neither of whom appear to be at all acquainted with the conditions of the sick room or the wards of a hospital. The views entertained by ourselves on this question have from time to time been placed before the readers of the Medical Press and Circular, and it is almost needless to say we are not content to receive the Panspermist's view, but rather lean to Dr. Richardson's belief that the agency of germs in the propagation and dispersion of disease is not satisfactorily accounted for by those who have consistently advocated the "germ theory." If we are expected to believe that the air we breathe is charged with invisible germs of disease, which come and go, we know not how, but which have an unlimited power of development and

multiplication, and possess great persistency of life, it is hard to see why there should not be an increase of them, and from which, as Dr. Richardson says, "there could be no escape for man or animal, and by which in time the world would be depopulated." If germs or exciting particles of disease really live, and under certain conditions, multiply to any extent in the body, how can medical skill ever hope to successfully battle with these hidden foes. It is indeed difficult to understand why a patient suffering from the specific germs of fever ever recovers, there can be no intelligible reason given why they should cease to multiply as long as the patient continues to live, and afford food for their sustenance. There is, in short, an air of extreme improbability in the germ theory. The sudden cessation of one disease, the periodicity of another, and the various phenomena connected therewith and with all epidemic conditions seem to oppose almost insuperable obstacles in the way of believing that living organisms are the cause of specific or epidemic contagious diseases. If we go further, and attempt to assign a cause for instance to cholera, we at once find that we have to deal with a migrating organism, a wandering form of disease not confined to hospital, house, or district, but capable of being conveyed along vast tracts of country, and affecting a body of men with a varying degree of intensity on one side of a river, and wholly avoiding a portion of the same body on the opposite side. A general fertilising diffusion of self-propagating living organisms entering the body as the air may enter, could hardly be expected to select a small minority of a population; and if it did so at the first why should it do so after it had seized upon many centres in which it could increase? It is indeed hardly conceivable that vital germs which under ordinary circumstances may be comparatively harmless, will at other times generate products of a virulently poisonous nature to the human body. Dr. Richardson not only entirely disbelieves in the agency of living propagating germs, but in their very existence, and he therefore advocates a glandular origin of disease—and by means of which the poison of communicable diseases are spread abroad. His doctrine is that the poison of scarlet or other fever is in no way connected with a germ, but a diseased secretion, indebted for any vitality it may possess, to the fact that it is a product, although a perverted one, of vital action. For a fuller exposition of this doctrine we refer our readers to the abstract of his paper.—*Medical Press and Circular, Oct.* 10, 1877, *p.* 302.

4.—ON THE ACTION OF THE SEPTIC EXTRACT OF MUSCLE.

(By Dr. J. Burdon-Sanderson, LL.D., F.R.S., Professor of Physiology in University College, London.)

The experimental inquiry, of which an account is given in the following pages, was undertaken for the purpose of obtaining more precise information as to the nature and properties of the poisonous substance to which septic liquids owe their virulence.

It has been long known that the aqueous infusions of putrid animal tissues, as, for example, flesh, produce, when introduced into the circulating blood of the higher animals, certain characteristic febrile and other symptoms which are recognised as those of septic infection. These symptoms were first studied early in the present century by Gaspard, and subsequently by Magendie. In more recent times they have been made the subject of more precise and extended investigations by Panum, by Otto Weber, and finally by Bergmann and his pupils.

Bergmann was led by his researches to infer that the septic poison is a chemical body held in solution by the water of infusion, and accordingly undertook, in co-operation with his colleague Professor Schmiedeberg, researches with a view to its separation by chemical methods. The results of those inquiries are well known. A product was obtained which was believed to be a pure substance, and called *sepsin*. The most characteristic fact relating to it was that when its solution was introduced into the living organism, it produced a pyrexia of several hours' duration, of which the temperature curve corresponded so closely with that of the pyrexia produced by the injection of small doses of septic liquids, that the identity could not be mistaken. This curve has been since often referred to by pathological writers as the sepsin curve.

Twelve years before Bergmann wrote, the subject had been investigated by Professor Panum of Copenhagen: as however his researches were published only in Danish, they failed to exercise their due influence.

In my researches on contagium, published in 1870, I had arrived at the conclusion, which I think I may venture to say has now been very generally accepted, that every contagium consists ultimately of particles not soluble in water. It being admitted that the deleterious agent in septichæmia does not consist of living organisms, it appeared to me possible, and indeed for several reasons probable, that it would be found to resemble the contagia in being particulate, *i.e.*, that the material, although in the ordinary sense soluble in water, might yet consist of particles so minute as to be capable of rendering the liquid in which they are suspended opalescent,

and to be irremovable by any of the ordinary methods of filtration.

In order to determine this important question I filtered liquids prepared in the same way as those of Panum through porous porcelain under pressure, employing a method which will be fully described in the following pages. The result was so far in accordance with my expectations that I found that changes were produced in the liquid extract which were of such a nature as to deprive it simultaneously of its offensive smell, its poisonous action, and of its remarkable power of generating bacteria. That these three changes stood in anything more than concomitant relation to each other cannot of course be asserted; there is no sufficient ground for regarding them as attributes of one and the same substance.

Before filtration, the extract, notwithstanding that it is perfectly limpid, always exhibits under high powers of the microscope, certain sharply-defined particles. These bodies, which are of such extreme minuteness as to be only just within the limits of microscopical visibility, present no recognisable constant characters either of form or size, so that I am entirely unable to attribute to them any morphological significance. They are entirely absent in liquids which have been passed, even once, through porous porcelain.

The fact has now been proved that virulent septic liquids, even when absolutely free from bacteria or their germs, can be deprived of their virulence by filtration through porous porcelain, but the greatest caution must be used in drawing inferences from it.

The only safe form indeed in which any conclusion can be expressed on the subject is the negative. From the mode of preparation, it can scarcely be supposed that the agent is a living organism. It can further be stated that it is not an albuminous compound, for the absence of bodies of this class is proved by the chemical properties of the liquid before filtration. Its complete removal by filtration renders it probable, but by no means certain, that it is not a crystalline body, soluble in water. The only group of bodies to which its properties suggest comparison is that of the so-called "unformed ferments."

Chemical, Physical, and Organic Properties of the Extract.—Experimental Investigations.—Preparation.—A quantity of finely-divided fresh muscle was macerated in water for fourteen days. The liquid was then strained off and evaporated to dryness on the water bath. The residue, reduced to a fine powder, was digested for an hour or more in seven parts of cold methylated spirit, and afterwards boiled for ten minutes in the spirit. It was then collected on a filter and carefully dried at a temperature of 80° C. until it had entirely lost the alcoholic odour.

The insoluble alcoholic residue thus obtained was digested for an hour in from eight to ten parts of cold distilled water, and filtered immediately before use.

Chemical Character.—When prepared in the above manner the liquid is nearly limpid and of a pale straw tint. It has a disagreeable odour and a slightly acid re-action. Its specific gravity is about 1,024, and it yields on evaporation at 100° C. from two to two and a half per cent. of solid residue. When heated it becomes slighly turbid or milky, but on the addition of a few drops of nitric acid again becomes perfectly clear. The addition of acetic acid and potassium ferrocyanide produces no reaction. Heating the extract after adding a few drops of nitric acid slightly deepens the colour, and the addition of a few drops of ammonia, after cooling, causes a deposit which, under the microscope, is seen to consist of feathery crystals, having all the characters of ammonio-magnesic phosphate. None of the other tests for albuminoid substances gave any indication of their presence.

Organic Characters.—The freshly-prepared liquid contains no bacteria, but a greater or less number of very minute shining spheroidal motionless bodies are constantly found in it, to the presence of which its faint opalescence is doubtless due. Exposure to the air, even for a very short time, is sufficient to induce the presence of bacteria. When the extract was kept in sealed tubes, at a temperature of 38° C., bacteria developed in it with extraordinary readiness.

It thus appears that the septic extract is remarkably fertile in relation to bacteria, *i.e.*, that these organisms originate in it with very great facility. In this respect it resembles those artificial cultivating liquids containing peptones which have been lately employed by Huizinga, Gscheidlen, Samuelsohn, myself, and others in experiments relating to the question of spontaneous generation. That, when freshly prepared, it is free from living organisms can scarcely be doubted, for not only does the most careful microscopical examination fail to bring any such organisms into view, but the material from which the extract is prepared has been immediately before thoroughly boiled in alcohol, a process which bacteria can scarcely be supposed capable of surviving.

In order to ascertain whether the extract, although free from living bacteria, owed its fertility to its containing particles of such minuteness as not to impair its transparency, the following experiments were made:—

First Series.—1. Two tubes, about four inches long, were drawn out and heated in the flame of the blow-pipe to a red heat. They were then sealed hermetically in the usual way. On cooling they were partially filled with the extract by break-

ing off their finely-drawn ends under the surface of the liquid, and were immediately sealed up by the blow-pipe. No. 1 was then placed in the warm chamber, at a temperature of from 35° C. to 38° C. Its contents were perfectly transparent. At the end of twenty days the liquid had become turbid, and on opening the tube the gaseous contents escaped explosively. The liquid was still slightly acid; it was very fetid, and teemed with organisms, namely, short rods, dumb-bells, and spheroids. Tube No. 2 was, previously to being placed in the warm chamber, heated to 110° C. and became slightly milky. On being opened at the same time as No. 1, there was no evolution of gas, and the liquid remained free from organisms.

Second Series.—Four vacuum tubes were prepared in the same manner as those used for the former experiment, and precisely the same method of filling and sealing was adopted. No. 1 was filled with freshly-prepared pyrogenic extract. No. 2 contained the extract filtered once through a porous cell. No. 3 contained extract which had been filtered twice through a porous cell. In No. 4 was extract which had been passed three times through a porous cell.

These four tubes were placed in the incubator at a temperature of 38° C., and were examined on the tenth day. No. 1 exploded slightly, when its neck was broken off. The liquid was turbid, and filled with actively-moving organisms, viz., short rods, dumb-bells, spheroids, and a few short chains. No. 2 exploded but very slightly. To the naked eye its contents did not present any marked turbidity, but when examined microscopically a small number of the same kind of organisms was found as in No. 1. Tubes 3 and 4 contained no organisms.

In order to determine whether the extract, though itself free from living bacteria, possesses, even in the fresh state, the power of determining their development in other liquids, the following experiments were made by the method of impregnation.

The cultivation liquid used was serum. In order to obtain it under conditions which would, as far as possible, preclude any accidental impregnation of it by means of organic germs, the following plan was adopted:—A glass tube, closed at one end, heated in the flame of the blow-pipe, and having its orifice plugged while cooling with cotton wool, was used for the collection of blood from the artery of a rabbit. During coagulation the plug of cotton wool was again inserted. On the following day the serum (about 8 c. c.) was collected and placed in four test tubes, which had previously been heated in the flames of a Bunsen lamp. Three of these tubes were then impregnated with a single drop of the pyrogenic extract, and their orifices were closed with cotton wool. The fourth tube was not im

pregnated, but the orifice was closed in the same manner as the others. The tubes were then placed vertically in a shallow dish containing a little water, covered by a bell jar, and introduced into the incubator at a temperature of 30° C.

At the end of two days the three impregnated tubes were quite turbid, while the unimpregnated one remained clear. In the former, microscopical examination revealed the presence of a great number of organisms, viz., spheroids, rods, dumb-bells, and chains. In the latter no organisms could be detected.

Physiological Action.—In the investigation of the physiological or toxical action of the pyrogenic extract, dogs were exclusively employed, for the following reasons: I had ascertained by preliminary experiments that in the dog the symptoms produced are remarkably constant as well as characteristic. In the smaller rodents (the only other animals at my disposal) I had found not only that the effects are inconstant, but that in these creatures the febrile process differs in respect of features more or less essential, from the same process in man; and in particular that, for reasons fully explained in my paper on fever, temperature cannot be used in them as a criterion of the intensity of the febrile disturbance.

The mode of introducing the pyrogenic extract into the circulation was as follows:—A subcutaneous vein having been exposed to the extent of about an inch, the flow of blood was arrested in it temporarily by compression with the finger, the vein wall was pierced by the canula of a syringe for hypodermic injection, of which the capacity was 5·16 cub. cent., and the requisite quantity was injected.

If more than 5 cub. cent. were required, the syringe was disconnected and refilled without disturbing the canula. After the injection the cutaneous incision was brought together with sutures, and always healed satisfactorily.

As the purpose of this investigation was to determine to what extent the activity of the septic extract is dependent on constituents which can be removed by mechanical means, it will be convenient first to give an account of the symptoms produced by the clear extract when prepared as above described, and repeatedly filtered in the usual way through paper, and the same liquid after it has passed through porous porcelain under pressure.

Toxical Action of the Filtered Extract.—The symptoms produced in dogs by the injection of pyrogenic extract were as follows:—The first symptom was shivering, more or less marked, continued for several hours, and accompanied by a rise of temperature amounting to 2° C., or even 3° C., the maximum being usually reached at the end of the third hour after the injection. Bilious vomiting and mucous diarrhœa were

always observed about the time at which the temperature was highest, and continued for some time after it began to fall. In addition to the mucous diarrhœa there was frequently a considerable amount of intestinal hemorrhage, and it was characteristic that the introduction of the thermometer into the rectum produced on each occasion tenesmus. In most cases thirst constituted a marked symptom, but if the animals were allowed to drink much water the retching and vomiting were greatly increased, and the water was brought up again almost as soon as swallowed. Usually after the end of the third hour, but never later than the end of the fourth hour after the injection, the symptoms began to subside, the temperature fell almost to the normal, and the vomiting and diarrhœa ceased. In the course of five or six hours, with the exception of an occasional slight shiver and some languor, the animal appeared to have recovered. In less than twelve hours after the injection the animals had become quite lively, and were anxious for food.

By way of example, the following case, in which a full grown and healthy animal was employed, may be given as very fairly representing the other experiments.

A healthy full-grown dog, weighing 8,264 grammes, was infected by injecting 10·32 cub. cent. of pyrogenic extract into the saphena vein in the manner already described. His temperature, previously to the injection, was 38°·7 C. The injection was completed at 4 o'clock, P.M., and immediately after, the animal became very restless, was salivated, and appeared to suffer from nausea. One hour after the injection the temperature had risen to 40°·1 C., and on withdrawing the thermometer from the rectum a small quantity of mucus was passed. The animal appeared very ill, was shivering, and at each inspiration uttered a peculiar whining moan. At 6 P.M. the temperature had increased to 40°·4 C., and now there was well marked mucous diarrhœa with tenesmus. The highest temperature, viz., 41° C. was observed at 7 P.M., all the other symptoms, but especially the shivering, being well marked. Four hours after the injection the temperature had fallen to 40°·4 C., but the other symptoms continued, and vomiting had also occurred. During the fifth hour the temperature further declined to 39°·9 C., the shivering was subsiding, the respiration had assumed a normal character, but the animal still appeared suffering. He was very dull, and refused food, stood with his back arched and tail depressed, his nose was hot and dry. On the following morning his temperature was normal, and except that he was somewhat dull, he appeared quite recovered.

Action of the Extract after Percolation through Porous Porcelain.—In the first experiment the Dog No. I. was employed, in which, as before stated, the character of the symptoms pro-

duced by an ordinary dose (10 centims.) of the fresh extract had been precisely ascertained. This animal received in all four doses of the extract, of which the first, second, and fourth were filtered in the ordinary way, the third through porous porcelain. The results were as follows :—

No. of Animal.	Date.	Weight of Animal.	Quantity injected.	Extract.	Normal Temp.	Temperature after injection. 1 hour	2 hours.	3 hours.	4 hours.	Symptoms.
I.	Aug. 28, 1874	grms. 8·264	c c. 13.32	Unfiltered	38·7	40·1	40·4	41·0	40·4	Shivering and vomiting. Temp. at 5th hour, 39·9.
	Sept. 1, 1874	—	,,	,,	38·6	39·1	40·6	40·2	39·4	Shivering very much
	,, 9, ,,	—		Filtered	38·4	39·2	38·5	38·3	38·4	No symptoms produced
	,, 14, ,,	—	,,	Unfiltered	38·8	32·9	41·4	41·5	40·8	Shivering and tenesmus. Temp. at 5th hour, 40·2.
II.	Dec. 30, 1874	5·329	8·7	Unfiltered	38·8	39·4	40·5	41·6	41·2	Vomiting and shivering
	Jan. 7, 1875	—	,,	,,	38·8	40·0	40·6	41·4	41·6	Shivering and mucous diarrhœa.
	,, 14, ,,	—	,,	,,	38·5	40·2	41·0	41·4	41·0	Do. do.
	,, 2, ,,	—	8·7	Filtered	39·1	39·3	39·0	38·8	—	No symptoms observed.
	,, 11, ,,	—	,,	,,	38·4	39·0	39·3	38·8	38·8	Do. do.
III.	,, 2, ,,	4,876	8·7	Unfiltered	39·6	40·7	41.2	40·7	40·4	Vomiting and shivering.
	,, 11, ,,	—	,,	,,	38·6	39·6	40·4	40·8	40·6	Salivation, vomiting, and dysenteric evacuations.
	Dec. 31, 1874	—	8·7	Filtered	38·7	39·0	39·2	39·3	39·0	No symptoms produced.
	Jan. 7, 1875	—	,,	,,	38·8	39·6	39·8	39·6	39·2	Do. do.
	,, 14, ,,	—	,,	,,	39·0	39·4	39·8	40·0	—	Do. do.
IV.	Feb. 19, 1876	7,030	9	Unfiltered	39·0	39·4	40·7	41·2	40·4	Shivering.
	April 1, ,,	—	10·32	,,	39·0	39·4	40·6	40·3	—	Diarrhœa and vomiting.
	,, 8, ,,	—	15·48	,,	39·1	40·1	40·6	41·0	—	Do. do.
	Sept. 16, ,,	—	9	Filtered	39·2	38·6	38·8	39·0	—	No symptoms produced.
	April 4, ,,	—	15·38	,,	39·2	39·3	39·2	39·2	—	Do. do.

The transitory and very inconsiderable rise of temperature which followed the third injection in Dog I. is of no significance,

for it is known by observation that it is not possible to inject 10 centims. of even the most indifferent liquid without producing this slight disturbance. It is to be noticed that by the end of the second hour the temperature had returned to the standard of health, and throughout the whole period of observation the animal was in an absolutely normal condition. *It was lively, answered readily when called, and showed no indication of being in the slightest degree affected by the injection.*

No. of Animals.	Date.	Weight of Dog.	Quantity injected	Extract.	Temperature. Normal.	1 hour.	2 hours	3 hours	4 hours.	Symptoms Produced.
1.	March 1, 1876	grms. 7,030	c.c. 8	Unfiltered	39·0	40·6	40·9	41·4	41·6	Vomiting and tenesmus.
	,, 17, ,,	—	15·48	,,	39·2	40·2	41·1	41·2	41·4	Vomiting, diarrhoea, and dysenteric evacuations.
	,, 16, ,,	—	,,	Filtered	39·0	39·6	39·2	39·2	—	No symptoms.
2.	March 7, 1876	7,937	10·32	Unfiltered	39·0	39·8	41·0	41·4	40·8	Bilious vomiting and diarrhoea.
	,, 10, ,,	—	10·32	Filtered	38·6	39·6	39·2	39·2	39·0	No symptoms.
3.	March 7, 1876	6,350	8	Unfiltered	39·2	40·2	39·8	40·2	—	Vomiting and tenesmus.
	,, 10, ,,	—	8	Filtered	39·1	39·8	39·4	39·2	39·2	No symptoms.

The result of three similar series of experiments are exhibited in the same table. They prove in the plainest way that whatever may be the nature of the poisonous agent, it admits of complete removal by mechanical filtration. By way of supplement to this observation, two series of experiments have been recently made in which an ordinary infusion of putrid muscle was substituted for the aqueous extract of the alcoholic precipitate. I subjoin the tabular summary of these experiments in the same form as the others. They have no bearing on one of the questions discussed in the introduction to this paper, viz., that of the participation of living bacteria in the production of the toxical action; for in this case the liquid swarmed with these organisms. But they exhibit quite as clearly as the other experiments, that if the body to which putrid infusion owes its poisonous action is soluble in water at all, it is so in such a sense that it is unable to pass through porcelain.—*Practitioner, July, p.* 19.

5.—GERM THEORY.—NOTES OF LECTURE BY PROFESSOR TYNDALL, F.R.S.,

At Royal Institution, London, June, 1877.

It is a well-known fact, that infusions of vegetable or animal substances, which when made are transparent, become in the course of a few hours, if kept at a proper temperature, turbid, and their sweet smell becomes putrid. This is owing to swarms of minute organisms which from their appearing in infusions are called Infusoria. The lowest class of these are called Bacteria.

There are two theories as to their origin. One, that they are developed from eggs or germs like the higher forms of animal and vegetable life; the other, that they arise spontaneously.

The chief advocate of the latter theory is M. Pouchet, of Rouen. His writings show how he arrived at his conclusions. He convinced himself by "meditation" that spontaneous generation was one of nature's modes of action, and then set to work to find evidence in support of his theory. His works are partly scientific, partly teleological, and partly satirical. He twits the "ovarists" with the difficulties of their theories. If there is a vast number of germs floating about in the air, as alleged by them, why cannot we see them? From their accounts they should form a thick dark cloud obscuring the sky. These theories were inconsistent with the idea of a creative wisdom.

However, Professor Tyndall said, it was an established fact that the blue of the sky, as seen from the highest elevations and above possibility of contamination with earth, was caused by vast numbers of foreign bodies floating in the atmosphere, so small as to be undistinguishable by a microscope magnifying by 1500 diameters. This had been the highest power available until Dallinger (to whom Professor Tyndall referred in terms of eulogy as a microscopist worthy of a far wider reputation than he had attained) had produced a power magnifying 15,000 diameters. This, however, failed to distinguish these minute germs. The only means of discovering their presence in the atmosphere was the electric light.

During his investigations or "battle with the germs," which occupied him for six months in 1875-76, he had found it to be a rule without exception that an infusion of turnip, cucumber, beef, or mutton, which had been boiled for a period of five minutes, would not putrefy in an atmosphere in which all germs had been allowed to subside. But during the continuance of these investigations in the autumn of 1876, he found that infusions apparently the same as those prepared in the previous year were not sterilised by boiling for fifteen minutes. There was no opposition, to his mind, between these results; the only question was, whence did the difference arise? Either

these infusions had, in October, 1876, a power of spontaneous generation which they had not in 1875, or there was a more obstinate contagium present in these which the former had escaped.

Acting on the assumption that the latter was the correct interpretation, he transferred his experiments from the Laboratory at the Royal Institution to the Jodrell Laboratory at Kew, in hopes of obtaining a purer atmosphere. The result was that five minutes' boiling at Kew was sufficient to sterilise infusions which had withstood boiling for 200 minutes at the Royal Institution. Either the infusions had lost a generative power at Kew which they possessed in the Laboratory, or there was a special contagium in the air of the latter place. Next he erected on the roof of the Royal Institution a shed in which he put his chambers. The infusions were carefully prepared in the shed, but the result was failure—the atmosphere in the shed was as bad as that in the Laboratory. It occurred to him that the shed might have been infected by his assistants passing to-and-fro between it and the Laboratory, and bringing the contagium with them. He therefore disinfected the shed by washing it with carbolic acid and water and caustic potash. He and his assistants wore proper uninfected clothes, and the result was that the infusions again became sterile after five minutes' boiling. A rod thirty feet long would connect the shed with the Laboratory. Had the infusions a generative power at one end of the rod, which they had not at the other end; or was the diffierence caused by a special contagium present in the Laboratory and not in the shed?

After exhibiting some specimens of infusions to illustrate the results of his experiments, the Professor pointed out the parallel between the spread of infection from the Laboratory to the shed, and the spread of infectious diseases in hospitals and other places by means of the passage to-and-fro of attendants.

The Professor then exhibited, by means of the electric light, the contagium in a sample of old hay brought from Heathfield, in Sussex, clouds of fine particles being seen to arise from the hay when shaken beneath the ray of light. This contagium was far more infectious than that ordinarily found in common air, and far more obstinate. The particles were extremely fine, and able to pass unaltered through 300 layers of filter-paper. This was the contagium which had infected the Laboratory, and was so remarkable for its resistance to heat.

If these were its effects on infusions of turnip or beef, what might its effects not be on open wounds in a hospital? This was a matter now being taken up by the followers of the Antiseptic School of Surgery, and was well worthy of the attention of all surgeons.

This difference between the powers of resistance of various species of contagia was of great moment with regard to the artificial preservation of meats and vegetables. He was not aware of any actual instance, but could imagine great financial reverses occurring to those engaged in these trades by infection from a contagium which would withstand the ordinary means of preservation from putrefaction. He knew that brewers were sometimes liable to checks from causes apparently inexplicable, and he thought that much might be traced to the special form of contagium. It would be possible to cause a great disaster by carrying a truss of hay like that which he had just exhibited through a preserving establishment or a brewery.

The Professor then referred to some tables exhibited on the boards above, giving a summary of the different periods for which he had subjected infusions of old hay, of turnip, and cucumber infected with hay dust, and of beef prepared in an infective atmosphere. The result appeared to be that the turnip and cucumber infusions could stand boiling for 180 minutes and yet putrefy. The beef putrefied after boiling for 300 minutes, and the old hay after boiling for 240 minutes. In one instance a sample of the hay infusion showed life after it had been boiled for 480 minutes—eight hours.

Boiling does not destroy the power of putrefaction possessed by any substance; it destroys, or is intended to destroy, the germs that are in the infusion or substance at the time. The germs that make an infusion putrefy are those in it, and not those in the air above. This the Professor had established by using a special form of bulb, which he was enabled to fill with purified germless air before he introduced the infusion. The infusion nevertheless putrefied, showing that it was the germs in it, and not any outside it, which were the cause of its putrefaction. When an infusion has been sterilised it may again be made putrefactive by introduction of fresh germs.

It was a grave error to confuse the germs of infusion with the adult forms. Heat destroyed the adult organisms, but the germs from which they sprang were comparatively indestructible. This was illustrated by the results of Professor Koch's researches on that dangerous and fatal disease, *Milzbrand*. He had found that an animal might with impunity take the adult organisms after they had been subjected to a very small amount of heat, but that the germs would withstand a lengthened period of boiling without losing their power of development. One minute's boiling will kill the adult, while eight hours is insufficient to kill the germ. It was not even necessary to raise the heat to boiling-point, for a heat of 145° Fahr. would kill the adult.

One result of his (Professor Tyndall's) experiments had been the method of disinfection by discontinuous heating. The substance to be disinfected should be subjected for one minute to a temperature of 140° Fahr.; this would kill all adult organisms. After a few hours' intermission, during which the substance is kept at a proper temperature, to enable the indestructible germs to arrive at a sufficiently sensitive stage of existence the substance should be again subjected to a mild heat. By this method an infusion would be more perfectly sterilised by an amount of heating which would in the whole amount to five minutes only, than by boiling for many continuous hours.

In one instance Professor Tyndall had noticed that an infusion contained in a sealed flask partially putrefied; a thick scum formed on the top, and the lower parts remained clear. From this and other reasons it had been inferred that bacteria resembled higher organisms in their dependence on oxygen for existence, and that in the present case the bacteria had crowded to the top of the liquid infusion to follow the air, and had thus stifled those beneath. He also showed an instance of a small quantity of a putrefying infusion which had quite exhausted all the oxygen in the large sealed flask in which it had been kept for some time.

Illusions from which air had been perfectly exhausted by means of the Sprengel air-pump had also remained sterile.

On the assumption that the mode of life of these lowest forms was the same as those of the highest, and knowing that it had been proved by experiment on the higher animals that an excess of oxygen acted as poison (an experiment which the Professor had never performed, and was not likely ever to perform, but of which he took occasion to say that he did not see how science was to make progress, and how diseases were to be combated without such experiments,) he tried the experiment of subjecting infusions of highly putrefactive matter such as cucumber and turnip to pressure of 200 atmospheres of oxygen, and found that they remained quite sterile. This result was not due to the mechanical pressure, but to the poisonous effects of the oxygen, for infusions subjected to a like pressure with common air had putrefied.

In conclusion, the Professor said that he hardly thought it necessary to summarise what had been there brought before his audience. In fact, the whole discourse was but a summing up of eight months of incessant labour. From the beginning to the end of the inquiry there was not, as had been seen, a shadow of evidence in favour of the doctrine of spontaneous generation: there was, on the contrary, overwhelming evidence against it; but he warned his hearers, not to carry away with them the notion sometimes erroneously ascribed to him, that he

deemed spontaneous generation impossible, or that he wished to limit the power of matter in relation to life. His views on that subject ought to be well known. But possibility was one thing, and proof was another; and when in the present day he sought for experimental evidence of the transformation of the non-living unto the living, he was led inexorably to the conclusion that no such evidence existed, and that in the lowest, as well as in the highest, of organised creatures the method of nature was that life should be the issue of antecedent life.—*Medical Times and Gazette, June* 16, 1877, *p.* 647.

6.—THE DOCTRINE OF CONTAGIUM VIVUM AND ITS APPLICATIONS TO MEDICINE.

By Dr. WILLIAM ROBERTS, F.R.S., Physician to the Manchester Royal Infirmary; Professor of Clinical Medicine in Owens College.

The notion that contagious diseases are produced by minute organisms has prevailed in a vague way from a remote age; but it is only within the last twenty years—since the publication of Pasteur's researches on fermentation and putrefaction—that it has assumed the position of a serious pathological doctrine. In the last decade, startling discoveries of organisms in the blood have given this doctrine the support of actual observation; and its application as a guide in the treatment of wounds by Professor Lister has made it a subject of universal interest to medical practitioners.

The resemblance between a contagious fever and the action of yeast in fermentation—or the action of bacteria in decomposition—is in many points so striking that it is difficult to avoid the impression that there is some real analogy between them. If, for example, we compare the action of yeast with small-pox, this resemblance comes out very distinctly, as the following experiment will show. I filled two pint bottles, A and B, with fresh saccharine urine, and inserted a delicate thermometer in each. A was inoculated with a minute quantity of yeast, but nothing was added to B. Both bottles were then placed in a warm place in my room, at a temperature of about 70° Fahr. In order to get a correct standard of temperature for comparison, I placed beside these a third bottle (C) filled with water, and inserted a delicate thermometer in it. All these bottles were carefully swathed in cotton-wadding, for the purpose of isolating their individual temperatures, and to obviate, as much as possible, the disturbing effects of the varying temperature of the room. For twelve hours no change took place; but, at the end of this time, A began to ferment, and the thermometer marked a distinct elevation of tempera-

ture. On the second day, A was in full fermentation, and its temperature was 2·7° above B and C. This disturbance continued for five days, the temperature ranging from two to three degrees above the companion bottles. The disturbance then subsided, and the temperature fell to an equality with B and C, and a considerable sediment, composed of yeast, settled at the bottom. In the meanwhile, B showed little alteration; but on the sixth day it began to ferment, the temperature went up, and for more than a week its thermometer stood about two degrees above A and C. Finally, the temperature in B declined, the disturbance subsided, and the newly-formed yeast settled to the bottom of the vessel.

This fever in a bottle resembled small-pox in the following points. A period of incubation intervened between inoculation and the commencement of disturbance; then followed a period of disturbance accompanied by elevation of temperature; this was succeeded by a subsidence of the disturbance and a return to the normal state. Great multiplication of the infective material (or yeast) took place during the process, and, after its conclusion, the liquid was protected from further infection with the same contagium. We likewise notice that the contagium of fermentation, like that of small-pox, may take effect either by direct purposive inoculation or by fortuitous infection through the atmosphere. In both cases the infective material has the power of preserving its activity for an indefinite period. The comparison fails in at least one important point—in the fermented urine, sugar is replaced by alcohol and carbonic acid; but we are not aware that any pronounced chemical changes occur in the blood or tissues during an attack of small-pox. I would, moreover, carefully guard myself against being supposed to suggest that the enhanced temperature in the fermenting urine is a real analogue of the preternatural heat of fever.

Let me direct your attention to another example—a kind of partial decomposition or fermentation which takes place in boiled hay-infusion when it is inoculated with the *Bacillus subtilis*. The *Bacillus subtilis* is a very common bacterium found in vegetable infusions and in curdling milk. I hope you will take note of this little organism; for I shall have to refer to it more than once in the course of this address. I took a flask containing hay-infusion, which had been sterilised by boiling, and inoculated it with a drop of fluid swarming with *Bacillus subtilis*. After the lapse of twenty-four hours the previously transparent infusion became turbid. This turbidity increased, and on the second day a film or crust formed on the surface of the infusion. On the third and subsequent days the crust broke up and fell in pieces to the bottom of the vessel.

In about a fortnight the turbidity passed away, and the original transparency of the infusion was perfectly restored, so that it looked exactly as it did before the process began, except that there was now a sediment consisting of the spores of the little organism at the bottom of the flask. In this case, again, there was the same succession of events; a period of incubation, followed by a period of disturbance, succeeded by a period of subsidence, and, finally, restoration to the normal state. There was also great increase of the infective material and immunity from further attack by the same contagium.

The yeast-plant and the *Bacillus subtilis* may be taken as representatives of a large class of organisms in regard to which we are only beginning to realise their vast importance in the economy of Nature and in the life of man. They are, as I shall presently show, the essential agents in all fermentations, decompositions, and putrefactions. We may group them together, for the convenience of description, under the general designation of *saprophytes*, a term intended to include under one heading all the organisms associated with the decomposition and decay of organic matter. The yeast-plant and its allies, and all the numerous species and varieties of bacteria, belong to this group. In size and form they are among the smallest and simplest of living things, but their vital endowments are wonderful.

All the organisms hitherto found associated with infective inflammations and contagious fevers belong to the tribe of bacteria, and we cannot advantageously enter on a study of that association without a knowledge of the origin and attributes of these organisms. This brings us into a field of active controversy. It has been alleged, as you know, on high authority, that these organisms, under certain conditions, depart entirely from the universal law of generation which is expressed in the aphorism *omne vivum è vivo*, and that they may arise spontaneously by a process of abiogenesis. It is also alleged that these organisms are not the actual agents of decomposition, but are merely associated with that process as secondary or accidental accompaniments. I propose to lay before you evidence that both these allegations are unsustainable, and to prove that bacteria, like other organisms, arise from pre-existing parent germs, and in no other way, and that they are the actual agents in all decomposition and putrefaction.

The first proposition I shall endeavour to establish is this: that organic matter has no inherent power of generating bacteria, and no inherent power of passing into decomposition.

I have here placed before you samples of three sets of preparations, out of a large number in my possession, which serve to substantiate this proposition.

The first set consists of organic liquids and mixtures which have been rendered sterile by a sufficiently prolonged application of the heat of boiling water. They are composed of infusions of vegetable and animal substances, fragments of meat, fish, albumen, and vegetables, floating in water. They are contained in oblong glass bulbs, and are protected from the dust of the air by a plug of cotton-wool inserted into the necks of the bulbs, but freely open to its gaseous elements, which pass in and out through the cotton-wool. They are all, as you see, perfectly transparent and unchanged, though most of them have been in my possession for several years.

The second set consists of organic liquids which have been simply filtered under pressure through unglazed earthenware into sterilised flasks. They include acid and neutralised urine, albuminous urine, diluted blood, infusions of meat and of hay. As these preparations were obtained by a method which is in some respects new, I will describe it to you. A piece of common tobacco-pipe, about six inches long, served as the filter. This was secured by indiarubber piping to the exit-tube of one of the little flasks used by chemists for fractional distillation. The flask is first charged with distilled water, and then a tight plug of cotton-wool is inserted into its neck. The flask is next set a-boiling briskly over a lamp. The steam rushes through the cotton-wool plug and through the tobacco-pipe, clearing both these passages of any germs they might contain. When the water has nearly boiled away, the end of the tobacco-pipe is hermetically sealed with melted sealing-wax. After a little more boiling, the flame is withdrawn and the neck of the flask is instantly closed with a tight vulcanite cork. The apparatus is now ready for action, and the tobacco-pipe is immersed in the liquid to be filtered. When the flask cools, a vacuum is created within it, and this serves as a soliciting force to draw the liquid through the earthenware into the flask. The process of filtration is very slow; it takes two or three days to charge the flask. When a sufficiency has come over, the apparatus is removed and placed on a shelf for a few days until the pressure inside and outside the flask is equalised. The vulcanite cork is then withdrawn, and the exit-tube is separated and sealed in the flame of a lamp. In this way you obtain a sterilised flask charged with the filtered organic liquid, and protected from outside contamination by a plug of cotton-wool. Preparations obtained in this way, if due precaution have been used in the manipulation, remain permanently unchanged; organisms do not appear in them, and decomposition does not ensue.

The third set of preparations are, in some respects, the most significant of the three. They consist of organic liquids which

have been simply removed from the interior of the living body, and transferred, without extraneous contamination, into purified glass vessels. I will not detain you with the methods employed to obtain them; it is sufficient to say that, by the use of proper precautions, it is possible to convey blood, pus, urine, ascitic fluid, pleuritic effusion, blister serum, or the contents of an egg, into sterilised glass vessels without contact with any infecting agency. Preparations thus obtained are exhibited in these flasks; they are protected from air-dust by a simple covering of cotton-wool. All of them are absolutely free from organisms and from any signs of decomposition.

What meaning can we attach to these preparations? You all know that liquids and mixtures such as these speedily decompose and swarm with organisms when left to themselves exposed to the air. They are of most varied composition, and the most apt of all known substances to breed bacteria and to become decomposed. They have been exposed to the most favourable conditions in regard to warmth, moisture, and air. Many of them have been in my possession several years, and all of them for several months, yet they are wholly barren and without sign of decomposition. I venture to say that these preparations substantiate in a positive manner the proposition with which we started—namely, *that organic matter has no inherent power of generating bacteria, and no inherent power of passing into decomposition.*

A second proposition is likewise established by these preparations—namely, *that bacteria are the actual agents of decomposition.* In all the preparations, the absence of bacteria coincides with the absence of decomposition. If I were to cause bacteria to appear in them, either by purposive infection or by exposing them to the unfiltered air, decomposition would infallibly follow. The filtration experiments supply a new and telling argument on this point. Some of the liquids became decomposed and full of bacteria while the filtration was going on, but the part which came over into the flasks remained without further change, showing that decomposition cannot go on without the actual contact of the living organisms.

We have next to ask ourselves, What are the sources and what is the nature of the fecundating influence which causes organic liquids, when abandoned to themselves without protection, to become peopled with organisms? In regard to their source, the answer is not doubtful. If I remove the covering of cotton-wool from any of these preparations, and admit unfiltered air, or a few drops of any ordinary water, however pure, or anything that has been in contact with air or water, organisms make their appearance infallibly in a few hours. As to the nature of the infective agents, we can say positively that they must consist

of solid particles, otherwise they could not be separated by filtration through cotton-wool and porous earthenware. Is it not a most natural inference that they are the parent germs of the brood which springs up at their impact? They are, however, so minute that we cannot identify them as such under the microscope; but Professor Tyndall has demonstrated that air which is optically pure—that is, air which is free from particles—has no fecundating power.

It is contended in some quarters that these particles are not living germs of any sort, but simply particles of albuminoid matter in a state of change which, when they fall into an organic liquid, communicate to it their own molecular movement, like particles of a soluble ferment, and so produce decomposition, which, in its turn, provides the conditions necessary for the abiogenic generation of bacteria. Filtration through porous earthenware furnishes a complete answer to this theory, for I found on trial that the soluble ferments passed with ease through the porous earthenware. If, therefore, this theory were true, the filtered liquids, if already commencing to be decomposed, would go on decomposing, and would develope bacteria after filtration; but instead of that they remain unchanged and barren. We are absolutely driven to the conclusion that these particles are living germs; no other hypothesis squares in the least degree with the facts of the case.

We may formulate this conclusion in a third proposition as follows:—*The organisms which appear as if spontaneously in decomposing fluids owe their origin exclusively to parent germs derived from the surrounding media.*

But how, you will ask, has it been possible, in the face of this evidence to maintain, with a show of success, the contrary opinion that bacteria can and do, exceptionally at least, and in certain media, arise spontaneously? This opinion is based on two undoubted facts, which, taken together, seem at first sight to stand in direct contradiction with the propositions I have enunciated. The first fact is that bacteria are invariably killed when exposed to a temperature of about 140° Fahr., or any higher temperature. The other fact is, that certain liquids, such as neutralised hay-infusion and milk, often produce bacteria after having been boiled—sometimes after having been boiled for two or three hours—and when there was no possibility of subsequent infection. It seemed at first sight a fair inference from these two facts that the apparition of organisms in boiled liquids was due to spontaneous generation, or abiogenesis. It does seem difficult to believe that any living thing can survive a boiling heat for several hours—and yet such is undoubtedly the truth. When I published on this

question in 1874, I advanced more than one line of proof which appeared conclusive that germinal particles of some sort did, under certain circumstances, survive a boiling heat; and that the instances referred to were examples of such survival, and not of a *de novo* generation. But I was not then able to explain the apparent contradiction involved in these experiments.

Since then, a new and surprising light has been thrown on this subject by the researches of Professor Cohn, of Breslau, and we are now in a position to offer a complete solution of the riddle. All the confusion has arisen from our having failed to distinguish between the growing organism and its seed or spore. You are all familiar with the immense difference in vital endurance between the seed and the growing plant. The same difference exists between a spore and its offspring. Some spores have an extraordinary power of resisting heat. Mr. Dallinger and Dr. Drysdale, in the course of their inquiries into the life-history of septic monads, demonstrated that while the living monads are killed by a heat of 140° Fahr., the spores of one variety, which are so minute that they cannot be seen, except in mass, by the highest powers of the microscope, are capable of germinating after being subjected to a heat of 300° Fahr. for ten minutes! If the spores of monads can resist this tremendous heat, there is no reason why the spores of bacteria should not be able to survive the feebler heat of boiling water. The development of bacteria in hay-infusion, after having been boiled continuously for several hours in hermetically sealed vessels, seemed to furnish the very strongest attainable evidence in favour of the abiogenic origin of these organisms; and yet, by a singular fatality, the investigations of Cohn have shown that this evidence, rightly interpreted, supplies a crowning argument against that view.

Cohn had the curiosity to examine the organisms which arose under these extraordinary circumstances. Did he find a new birth? On the contrary, he recognised a familiar form: none other than our old acquaintance the *Bacillus subtilis*. He followed it through all the stages of its development. It first appeared some twenty-four hours after the boiling, in the form of innumerable short moving rods. On the second day these rods shot out into long threads; on the third day there appeared on the threads, at perfectly regular intervals, strongly refractive oval bodies, which he identified as spores. Finally, the threads broke down and the spores were set free. In many hundred observations he saw this one organism and no other, and witnessed the successive stages of its development occurring with the constancy of a physical experiment.

Now, let me ask if this looks like an act of abiogenesis. The

evolutionist demands, for the transformation of one organic type into its next descendant, myriads of generations, and I know not what lapse of ages. But here, if this be a case of abiogenesis, we see accomplished at one leap, in a single generation, and in seventy hours, not merely the bridging over of the gulf between the dead and the living, but the development of a specifically distinct organism, with definite form, dimensions, and mode of growth, and furnished with a complete provision for the reproduction of the species! I need scarcely say that such a feat would be, not only without parallel in the history of evolution, but would be wholly contradictory to that theory.

The only group of bacteria, so far as is known, which form spores are the *Bacilli*; and Cohn remarks that in all the various cases in which he had observed organisms to arise in boiled liquids, they belonged in every instance to the *Bacilli*.

Before leaving this part of my subject, I wish to suggest certain considerations in regard to the nutrition and function of saprophytes, which appear to me to render it in the highest degree improbable that spontaneous generation should ever be discovered in this quarter. If it be assumed that the occurrence of abiogenesis, at some time in the past history of the globe, is a necessary postulate in science, then I see nothing unscientific—looking to the law of continuity in the operations of nature—in the supposition that it is occurring at the present day somewhere or other on the earth's surface, but certainly not in decomposing liquids.

Saprophytes are, as is well known, destitute of chlorophyll, and, like all such plants, they are unable to assimilate carbonic acid. They obtain their carbon exclusively from more complex compounds which have been prepared for them by pre-existing living beings. It is, therefore, manifestly impossible that the primordial forms of life could have belonged to this group; for if we throw ourselves back in imagination to that remote era when life first appeared on the globe, we should find ourselves in a purely inorganic world—amid conditions in which saprophytes could not possibly live nor obtain nourishment. The special function of saprophytes in the order of nature is to destroy, not to create, organic matter; and they constitute the last, not the first, link in the biological chain. For if we regard the order of life as it now proceeds on the earth's surface, we may describe it as beginning with the chlorophyll body, and ending with the saprophyte. The chlorophyll body is the only known form of protoplasm which obtains all its nutriment from inorganic sources: here integration is at its maximum, and disintegration at its minimum, and the resultant of the nutritive operations is increase of

organic matter. The saprophyte, on the contrary, feeds on nutriment prepared for it by other beings: here integration is at its minimum, and disintegration at its maximum, and the resultant of the nutritive process is decrease of organic matter. What takes place in a decomposing liquid, under the action of saprophytes, is progressive disintegration, and finally a breaking-up of all the organic compounds it contains into carbonic acid and ammonia; and the process ends with the mutual destruction of the organisms themselves. Organisms could not, therefore, begin in this way. The primordial protoplasm must have been either the chlorophyll body itself, or a body having a similar mode of nutrition.

If the search for contemporary abiogenesis is to be continued —as doubtless it must be, for science is insatiable,—it appears to me that the inquirer should endeavour to realise the conditions under which abiogenesis must have occurred in the first instance. For, if the process be going on amongst us at this day, it may be assumed as probable that it still proceeds on the original lines laid down at the dawn of life. If ever I should be privileged to witness an abiogenic birth, I should certainly not expect to see a saprophyte: I should rather expect to see a speck of protoplasm slowly formed, without definite shape or dimensions, and nourishing itself, like the chlorophyll body, on a purely mineral diet. The more one reflects on this subject, the more clearly does it appear that the spontaneous origin of saprophytes is logically impossible. Speaking as an evolutionist, I should rather infer that saprophytes were a late development—probably a degradation from some algal forms which had found their profit in feeding on waste organic matter, and which gradually lost their chlorophyll through want of use, and with it their power of feeding on an exclusively mineral diet.

We now approach the more practical side of our subject—that which concerns us as practitioners of medicine and students of pathology. I have already directed your attention to the analogy between the action of an organised ferment and a contagious fever. The analogy is probably real, in so far at least that it leads us to the inference that contagium, like a ferment, is something that is alive. We know of nothing in all our experience that exhibits the phenomena of growth and self-propagation except a thing possessed of life.

This living something can only be one of two things: either it is an independent organism (a parasite) multiplying within the body or on its surface, or it is a morbid cell or mass of protoplasm detached from the diseased body and engrafted on the healthy body. Possibly, both these conceptions may have their application in the explanation of different types of infec-

tive diseases. In regard to the latter conception, however (the graft theory), which has been so ably developed by my friend Dr. Ross, I will only say that it has not, as yet, emerged from the region of pure speculation. It lacks an established instance or prototype, and it fails to account for the long-enduring dormant vitality so characteristic of many contagia, which conforms so exactly with the persistent latent vitality of seeds or spores, but which contrasts strongly with the fugitive vitality of detached protoplasm.

If, then, the doctrine of a contagium vivum be true, we are almost forced to the conclusion that a contagium consists (at least, in the immense majority of cases) of an independent organism or parasite; and it is in this sense alone that I shall consider the doctrine.

It is no part of my purpose, even if I had the time, to give an account of the present state of knowledge on this question in regard to every contagious disease. My object is to establish the doctrine as a true doctrine; to produce evidence that it is undoubtedly true in regard to some infective inflammations and some contagious fevers. In an argument of this kind, it is of capital importance to get hold of an authentic instance; because it is more than probable—looking to the general analogy between them—that all infective diseases conform in some fashion to one fundamental type. If septic bacteria are the cause of septicæmia—if the spirilla are the cause of relapsing fever—if the *Bacillus anthracis* is the cause of splenic fever—the inference is almost irresistible that other analogous organisms are the cause of other infective inflammations and of other specific fevers.

I shall confine my observations to the three diseases just named—septicæmia, relapsing fever, and splenic fever; merely remarking that, in regard to vaccinia, small-pox, sheep-pox, diphtheria, erysipelas, and glanders, the virus of these has been proved to consist of minute particles having the character of micrococci; and that, in regard to typhus, scarlet fever, measles, and the rest of the contagious fevers, their connexion with pathogenic organisms is as yet a matter of pure inference. For further details I must refer you to the able reports of Dr. Braidwood and Mr. Vacher on the Life-History of Contagium, made on behalf of this Association, and published in the Journal in the course of the past and present years.

Septicæmia.—We will first inquire how it stands with this doctrine in regard to traumatic septicæmia and pyæmia. You are all aware that foul, ill-conditioned wounds are attended with severe, often fatal, symptoms, consisting essentially of fever of a remittent type, tending to run on to the formation of embolic inflammations and secondary abscesses.

The notion that septicæmia is produced by bacteria, and the *rationale* of the antiseptic treatment which is based thereupon, is founded on the following series of considerations:—

1. It is known that decomposing animal substances—blood, muscle, and pus,—develope, at an early stage of the process, a virulent poison, which, when injected into the body of an animal, produces symptoms similar to those of clinical septicæmia. This poison is evidently not itself an organism; it is soluble, or at least diffusible, in water, and it is capable by appropriate means of being separated from the decomposing liquid and its contained organisms. When thus isolated, it behaves like any other chemical poison; its effects are proportionate to the dose, and it has not the least power of self-multiplication in the body. To this substance Dr. Burdon-Sanderson has given the appropriate name of pyrogen. It is the only known substance which produces a simple uncomplicated paroxysm of fever—beginning with a rigor, followed by a rise of temperature, and ending (if the dose be not too large) in defervescence and recovery.

2. We know further, from the evidence I have laid before you, that decomposition cannot take place without bacteria, and that bacteria are never produced spontaneously, but originate invariably from germs derived from the surrounding media. We are warranted by analogy in regarding pyrogen as the product of a special fermentation taking place in decomposing albuminoid mixtures, but we cannot name the particular organism nor the particular albuminoid compound which are mutually engaged in the process.

3. In the third place, we know that when a wound becomes unhealthy, as surgeons term it, the discharges become offensive —in other words, decomposed,—and when examined under the microscope they are found to swarm with organisms resembling those found in all decomposing fluids. Meanwhile the patient becomes feverish, and suffers from the train of symptoms which we call septicæmia.

It is a natural inference that what takes place in decomposing blood or muscle in the laboratory, takes place also in the serous discharges and dead tissues of the wound. These become infected from the surrounding air, or from the water used in the dressings, with septic organisms; on that follows decomposition and the production of the septic poison, or pyrogen; the poison is absorbed into the blood, and septicæmia ensues.

It was the distinguished merit of Lister to perceive that these considerations pointed to a means of preventing septicæmia. He argued that if you could prevent the access of septic organisms to the wound, or destroy them there, you

would prevent decomposition, prevent the production of the septic poison, and thus obviate the danger of septicæmia. It is not within the scope of this Address to describe the means by which Lister attained this object, still less to pass judgment on his practice, but I may be permitted to express my belief that the principle on which the treatment is founded is unassailable.

We should probably differ less about the antiseptic treatment if we took a broader view of its principle. We are apt to confound the principle of the treatment with Lister's method of carrying it out. The essence of the principle, it appears to me, is not exactly to protect the wound from the septic organisms, but *to defend the patient against the septic poison.* Defined in this way, I believe that every successful method of treating wounds will be found to conform to the antiseptic principle, and that herein lies the secret of the favourable results of modes of treatment which at first sight appear to be in contradiction to the antiseptic principle. Take, for example, the open method of treating wounds, which is sometimes compared in its results with Lister's method. What is this treatment but another way (only less ideally perfect than Lister's) of defending the patient against the septic poison? Because, if the surgeon succeeds in providing such free exit for the discharges that there is no lodgment of them in the wound, either they pass out of it before there is time for the production of the septic poison, or, if any be produced, it escapes so quickly that there is not enough absorbed to provoke an appreciable toxic effect.

Before we can understand the pathology of septicæmia, we must have clear ideas on the relation of septic bacteria to our bodies. We see in our laboratories that dead animal tissues, when exposed to ordinary air or ordinary water, invariably breed septic organisms; in other words, contact of the septic germs with the dead tissues never fails to produce successful septic inoculation. But it is quite otherwise with the same tissues when alive and forming part of our bodies. You cannot successfully inoculate the healthy tissues with septic bacteria. It has been proved over and over again that these organisms, when separated from the decomposing medium in which they grow, can be injected in quantity into the blood or tissues of a healthy animal, or applied to a sore on its skin, without producing the least effect. The healthy living tissues are an unsuitable soil for them; they cannot grow in it; or, to put it in another way, ordinary septic bacteria are not parasitic on the living tissues.

This fact is of fundamental importance in the discussion of the pathology of septicæmia. We have a familiar illustration

of its truth in the now common practice of subcutaneous injection. Every time you make a subcutaneous injection you inject septic germs into the tissues. I had the curiosity to test this point with the morphia solution used for this purpose in the Manchester Infirmary. I injected five drops of this solution into four flasks of sterilised beef-tea, which had remained unchanged in my room for several months, taking care to avoid any other source of contamination. In forty-eight hours they were all in full putrefaction. But we know that no such effect follows when similar injections are made into the bodies of our patients.

It seems also probable that septic organisms enter constantly into our bodies with the air we breathe and the food we take; they pass, presumably, like any other minute particles, through the open mouths of the lymphatics and lacteals, and penetrate some distance into these channels; they certainly come in contact with the accidental cuts, sores, and scratches which so often bedeck our skins. Notwithstanding all this, our bodies do not decompose; indeed, if ordinary septic organisms could breed in the living tissues as they do in the same tissues when dead, animal life would be impossible—every living creature would infallibly perish. How these organisms are disposed of when they do enter our bodies accidentally, as it were, in the various ways I have suggested, we cannot say; we can only suppose that they must speedily perish, for we find no traces of them in the healthy blood and healthy tissues.

Bearing in mind, then, that ordinary septic organisms cannot breed in the living tissues, unless, at least, they are reduced to near the moribund state; bearing also in mind that there is a sharp distinction to be drawn between the septic poison and the organisms which generate it, we are in a better position to consider the course of events in a wound which leads on to septicæmia and pyæmia. What probably takes place is this. An unprotected wound receives infection from the septic organisms of the surrounding media. If the discharges are retained in the sinuosities of the wound, decomposition of them sets in with production of the septic poison. This is absorbed into the blood, a toxic effect follows, and septicæmia is established. As this effect increases with the continuous absorption of the poison, the vitality of the system is progressively lowered, and especially the vitality of the tissues bordering the wound, which may be topically affected by the poison which percolates through them. These tissues at length become moribund or die outright; the septic organisms then invade and breed in them, more septic poison is produced and absorbed; the toxæmia becomes intense, embolic centres of inflammation and suppuration are formed, and the end comes. In all this

history there is no necessity to assume, nor even a probability, that septic organisms invade, or at least multiply in, the blood. They may do so at the near approach of death, but scarcely before that period.

In the course of traumatic septicæmia there sometimes occurs an event of great importance which imparts a new feature to the disease: I mean *infectiveness*. How this arises is a matter of speculation. To me it appears probable that, under a certain concurrence of conditions in and about the wound, a modification takes place in the vital endowments of the septic organism, whereby it acquires a parasitic habit, which enables it to breed in tissues of degraded vitality, or even in the healthy tissues, and in this way to produce the infective endemic pyæmia which we sometimes witness in the wards of our large hospitals.—*Medical Times and Gazette, Aug.* 11, 1877, *p.* 138.

7.—ANTIPYRETIC TREATMENT OF ENTERIC FEVER.

By Shirley F. Murphy, Esq., Resident Medical Officer to the London Fever Hospital.

Cold bathing in enteric fever is not attended with any difficulty. In the London Fever Hospital a bath has been constructed on wheels, so that it can be readily placed by the side of the patient's bed. It is six feet in length, and slopes gradually from the head to the hips; the patient is in this way supported with his face above the water-level, while the rest of his body is completely immersed. A tap at the foot of the bath will rapidly empty it. On to this an india-rubber hose is screwed, of sufficient length for the other end of the hose to be placed in a sink in an adjoining lavatory. Two more hoses, connected with hot and cold water taps in the lavatory, are long enough to reach the bath at any part of the ward, and serve to fill it and regulate the temperature of the water, which should vary according to the condition of the patient, those especially feeble or with weak hearts requiring a temperature of 80°, which must subsequently be reduced to 70° or 65°, if the patient stand the bath well. The majority of patients are placed in a bath of 60° to commence with. The temperature of the patient is taken in the rectum, and his pulse noted immediately before immersion. He is then wrapped in a sheet and lifted into the bath. Another sheet is then stretched over the bath, and the one surrounding the patient withdrawn, so that the water circulates freely round his body. This is a matter of some importance, for if the sheet be left round the patient, the water inside it soon becomes warm, and the cooling process does not go on. His head and face are immediately sponged, and as much of his head kept under water as can be managed without the water being permitted to enter his

mouth and nose. His tongue and mouth should be cleaned at the same time. In those cases in which the patient has been placed in a bath of 80°, the temperature is reduced to the point required. The length of time he remains in the bath depends on the height of his temperature and that of the water. If the water be at 60°, from fifteen to twenty minutes will be sufficient, but in those cases where it is 80° to commence with, and has subsequently been reduced to 70°, half an hour's immersion is required. While the patient is in the bath, his bed is being prepared for his reception. A mackintosh is spread over the bottom sheet, and over this a blanket. When he is removed from the bath he is, at the moment of being lifted, covered with the sheet previously stretched over it, and is then placed on the bed and wrapped in the blanket on the mackintosh, the wet sheet at this moment being withdrawn. In this manner the patient may be bathed without the least exposure of his person. He is left for half an hour in the blanket, at the end of which time it and the mackintosh are withdrawn. Occasionally a little brandy, before and during the bath, is given; but this of course depends on the condition of the patient, and is required only when he is very weak.

At the moment of being placed in the bath the patient is usually a little nervous, but this passes off as soon as he is well under the water; even a delirious patient rarely offers resistance when once thoroughly immersed. He remains quietly in the water for about ten minutes and then begins to complain of the cold and to shiver. As a rule the temperature in the rectum does not commence to fall until this time, and in severe and advanced cases not until after twenty minutes or half-an-hour's immersion. The effect of the bath in the first five minutes is usually to slightly increase the temperature in the rectum; and often in severe cases the temperature does not fall more than one or two degrees in the first half hour of immersion. When it has once fallen to this extent it falls as much as four or five degrees in the next ten minutes. In exceptionally severe cases three-quarters of an hour is required to reduce the temperature two or three degrees; after this it falls suddenly as much as five or six degrees in the next few minutes. It is, however, unnecessary to do more than reduce the temperature two degrees while in the bath, as it usually continues to fall for an hour or more after the patient's removal. If the patient be taken from the bath at the end of ten minutes or a quarter of an hour after immersion, it is often found that the temperature in the rectum is as high as it was before he was placed in the bath, but that subsequently a fall of three or four degrees takes place. The rapidity with which the temperature falls and the length of time it remains low entirely depend on

the period of the disease and its severity; in mild and early cases the temperature may not attain its previous height for twelve hours, whilst in severe and late cases it will often do so in two hours.

The effect of the bath on the pulse is to quicken it at the moment of immersion, but this increase immediately passes off, and in ten minutes or a quarter of an hour the pulse is reduced by eight or ten beats in the minute. After a longer time, such as half an hour or more, the pulse becomes more rapid, and continues to increase the longer the patient is in the bath; but if he be put to bed after a quarter of an hour's immersion, the reduction in frequency is not only maintained, but the pulse is still further lowered. The longer the patient remains in the bath the more is the pulse diminished in volume, and after half an hour's immersion it is often hardly perceptible at the wrist.

Nothing can be more striking than the influence of the bath on the nervous system. Although unconscious or delirious before the bath, after about a quarter of an hour's immersion the patient usually recovers consciousness. Nor is this the only beneficial effect; the bath is in a large majority of cases followed by quiet sleep, which may last an hour or two, or more. It has been observed by Dr. M'Combie, of the Homerton Fever Hospital, that after ninety-eight baths sleep followed in seventy-one cases; in twenty-one cases the patients dozed for some hours; and in two only did it fail to effect even the latter. The subsultus is diminished, and the whole aspect of the patient is changed. His countenance becomes clear, his expression intelligent, and his tongue (which is before dry and fissured) becomes moist and cleaner. Liebermeister has pointed out that sordes do not collect about the mouth under this treatment—a statement with which the experience of the London Fever Hospital entirely agrees. Another and most important effect is the increased ability of patients to take food.

Menstruation is in nearly every case suspended by the cold bath, but no bad result has been known to follow, the menstruation again commencing as soon as the baths are discontinued. The effect of the cold bath on the secretions is not fully known, but it has not been found to diminish the amount of urea excreted. Lung complications are no bar to its use, the only contra-indications being hemorrhage from the bowel and peritonitis, or collapse depending on perforation. A weak heart is only an indication for care and watchfulness, and of the necessity of a warmer bath at the commencement, with a more gradual reduction of the temperature of the water afterwards.

The consideration of when and how often the bath shall be

given is a matter of extreme importance, not only on account of the effect on the patient, but because its constant repetition is regarded as a bar to its use in private practice.

The bath produces its fullest effect when the temperature has reached its maximum, and is about to fall, or when it is already falling. This time varies somewhat in different patients, but is in a large majority of cases somewhere between eight in the evening and midnight. If the temperature of a patient suffering from enteric fever be observed every hour it will be found to commence to fall somewhere between these hours, and to continue to do so until nine or ten the following morning; it then rises slowly until noon, after which the rise becomes more marked until evening, when it again falls. The general rule to be observed is to bathe the patient whenever the temperature in his axilla is 102°, or in the rectum is 103°.

It has been already stated that in severe and advanced cases the temperature will regain in two hours the height it had attained before the bath. It is then necessary to again bathe the patient, but persistent bathing as often as this is seldom required after the first twenty-four or forty-eight hours of this treatment. The temperature then becomes more manageable, and can be regulated by a bath shortly after noon, and a second at nine or ten in the evening. The bath in the middle of the day prevents the rise of temperature at that time, and the bath late in the evening will ensure the temperature remaining low until the following day. It is seldom advisable to bathe the patient for a longer time than fifteen minutes. It has been found that baths for short periods frequently repeated are better borne than for longer periods at greater intervals. The increased frequency of the pulse after a long immersion must be regarded as a sign of prostration, and the patient should be removed from the bath before this takes place. Even if the bath be repeated every two hours a large portion of the interval is spent in sleep, and this repetition never produces exhaustion. It may, then, be stated briefly that, to commence with, the temperature should be taken in the rectum every two hours, and that whenever the temperature in the rectum of adults is 103°, or of children 104°, seeing that the latter bear high temperatures better than the former, a bath should be given lasting fifteen minutes, and that as soon as the temperature is under control a bath in the afternoon and in the evening will usually suffice for the rest of the illness.

For the night after the first day or two, and except in very severe cases, quinine may be wholly relied upon. As yet no mention of this has been made, for without the bath quinine will only control the temperature in the milder cases. In those more severe quinine is almost powerless to reduce temperature

until the patient has been several times bathed. It is then of the utmost value, not only in maintaining a low temperature after a bath, but also in again reducing the temperature if it has again risen. The influence of quinine on the temperature is very similar to that of the bath. It is first followed by a slight increase, and afterwards by a fall of three or four degrees. For this purpose it must be given in large doses; for an adult thirty grains of the powder is required, and should be given in two doses of fifteen grains with half an hour's interval between the two. The usual symptoms of singing in the ears, deafness, and occasionally vomiting, are produced, but are always borne well. When given immediately after a bath it serves to delay the next rise of temperature; but it is especially useful during the night, when the bath is attended with more trouble than during the day. Quinine will never altogether replace the bath, for it is not followed by the favourable effects of the latter on delirium and in producing sleep, but given in conjunction with the bath it is a most useful aid in antipyretic treatment. Sponging the patient with cold water or wrapping him in sheets saturated with cold water are useful in mild cases, or among patients who have been systematically bathed at the commencement of their illness, but they cannot be relied upon in severe cases until the tendency of the temperature to rise to a high point has first been conquered.

In conclusion it may be stated that, although the evidence of German writers is weakened by the want of separation of cases of febricula from undoubted enteric fever, their statistics show a considerable balance in favour of the antipyretic treatment; that although the cases treated in the London Fever Hospital are too few to urge as statistical proof of the efficacy of the treatment, the results on individual cases have been sufficiently satisfactory to lead to its general adoption in that institution. It is not contended that it will ensure the recovery of every patient suffering from enteric fever, that it will save life in those patients who die in the second week of their illness from the direct influence of the poison, nor that, when commenced late in the third week, it will undo the injury that has already occurred. It is not believed that it will shorten the period of illness; for it has even appeared to prolong it, although this may be due to the fact that the more severe cases have been bathed —cases which would under any circumstances have run a long course. But there is a large class of cases which, under the expectant treatment, die at the end of three or four weeks, worn out by the continued pyrexia, and these can without doubt be saved by an early and systematic antipyretic treatment.

The difficulties of the treatment are inconsiderable. An intelligent nurse can be trusted to take the temperature of

the patient. Any bath long enough for the patient to lie down in will answer the purpose, while its temperature can be easily regulated by the addition of a pail or two of hot or cold water. A little attention to the time at which the bath is given and to the use of quinine will enable any practitioner to give to it all the time the case requires without any great interference with his other duties. The treatment is not attended with any danger, the patients rarely object to it, and by some it is liked. The reduction of delirium, the quiet sleep, and the general feeling of comfort it gives the patient, are sufficient to convince the most sceptical that the cold bath is one of the most useful therapeutic agents we have in the treatment of enteric fever.—*Lancet, June* 2, 1877, *p.* 791.

8.—ON THE SANITARY CONDITION OF CHINA: SEWAGE, DRAINAGE, AND TYPHOID FEVER.

By Dr. John Dudgeon, Pekin.

China may be said, in a word, to be totally destitute of sanitary science. Take the following as a specimen of the condition of sanitation at Canton. Dr. Wang is speaking of typhoid fever, and he says he saw only two cases during a period of more than 10 years. He has seen many cases of remittent and intermittent fevers, but never one of typhoid. It may therefore be safely affirmed that this disease is not at all prevalent, although we should expect a different state of things, as the causes that are usually supposed to produce typhoid fever are in full operation. In Canton large numbers of the natives are daily using water and inhaling air charged with the impurities of human excreta, apparently with utter impunity. River water is greatly used, and that used by the boat population along the different jetties is extremely filthy, and must be largely contaminated with human and other impurities. They do not suffer from diarrhœa and fever more than others, but rather less. The filthiness of the creeks which ramify into different parts of the city are much worse than this. He gives one illustration of a creek near the foreign settlement which has been under his observation for some years. It is narrow, crowded with boats—innumerable houses on each side—the alvine dejections and other impurities of thousands of inhabitants along it are daily discharged into the stream, yet the water, too dirty even for washing, is daily used for culinary purposes without being filtered, or is precipitated with alum as is done elsewhere. Here we should expect the prevalence of such diseases as typhoid and diarrhœa among the inhabitants occurring often enough to excite attention, but their very impunity is one of the reasons for their continuing

to use the water. He adds, a detailed examination of this creek and the disgusting habits of the inhabitants would almost unsettle one's ideas of the connection between typhoid fever and polluted water. I can myself corroborate every word of this as witnessed at Tientsin, in the North, where a similar condition of things exists. Or take another witness, Dr. Reid, at Hankow, in Central China, on the Great River Yangtse. Speaking of the same fevers of the intermittent and remittent type which are so prevalent in China, the latter especially, he says—"The failure to discover any type of exanthematous fever is scarcely what might have been anticipated, knowing the filthy condition of the houses and streets, the density of the population, and the poverty in many quarters of the native city. It might have been presumed that the haunts of enteric fever at all events would have come to light, seeing that the products whence its organisms are supposed to be derived and nourished abound in many directions. This will be readily acknowledged if allusion be made to two of the more active and constant sources of impurity—viz., the emanations from the latrines and drains. The latrines are of course numerous—constructed without regard to cleanliness, aud nothing is used to interfere with the results of decomposition. Their contents are allowed to accumulate for three or four weeks until the large, deep open troughs underneath are filled, and they are then disposed of to the farmer or gardener and carried to the jetties in uncovered buckets, often during the day. While the process of emptying the troughs is going on the neighbourhood is saturated with odours of the most intense description, and which defy the tolerance of even well-blunted olfactories. The boys in an adjoining school, who never smelt fresh air, were obliged to have their nostrils stopped or compressed during successive days to exclude the stench. Notwithstanding the apparent undesirable character of the locality, in some cases private dwellings and even restaurants doing a thriving business, may be seen attached to the latrines, and only separated by a wooden partition insufficient to oppose the entrance of polluted air. The drains are blocked up with accumulations of mud and debris. From the want of means to obviate the regurgitation of gases from the cesspools of the streets, connected with the courtyard or interior of the houses, these drains must contribute largely towards increasing the impurity of the houses. In the poorer quarters the open ditches are half filled with decomposing refuse and garbage, and exposing their nauseous contents close by the doors or even under the floors of the houses." He further adds—"These various prolific sources for the development of organic germs have been specially referred to in connection with the absence

of enteric fever, because it may happen that in time evidence may be collected to prove or contradict an important theory in relation to this fever, that where malaria exists there is neutralisation or tolerance of the enteric poison." At Chefoo, Dr. Meyers reports "that the water is decidedly unsatisfactory, full of organisms, owing to the total want of circumscribed cemeteries and the water percolating through the soil. The surroundings of the wells, moreover, are filthy, combined with dirty buckets and dirty water-carriers. Persons long resident here appear to suffer but little inconvenience from this state of affairs. There is no drainage, but at the same time I must confess that this appears to exercise no injurious influence over the sanitary state. There is total absence of all those deleterious effects which might be justly supposed to follow so dangerous a disregard of sanitary laws." Amoy, according to Drs. Müller and Manson, is superlatively dirty, streets narrow, irregular, and filthy in the extreme, and redolent of every impurity. Pigs and dogs are the sole representatives of the elaborate machinery of sanitation in use in European towns, and a scientific sanitarian, with only home experience to guide him, would confidently predict the origin of epidemics and death. Yet the Chinese manage to live and thrive where he would hardly dare lodge his pigs. There is no typhus, no typhoid, or other disease, considered the inevitable consequence of defective sanitation, although Amoy, and indeed all Chinese towns, are full of typical typhus dens. Luckily filth, over-crowding, and bad food are not the only factors necessary for the manufacture of a typhus epidemic. Were they so we should live here (China) in perpetual dread. And these are not the only fevers whose absence we have remarked, with the exception of smallpox, we have met with no representative of the class of continued fevers which claims so large a number of victims in Europe. No case has been met with in either Amoy or Formosa of scarlet fever, measles, relapsing fever, or diphtheria. The petechial fevers, with the exception mentioned, are entirely awanting. Considering this and reflecting on the rarity of the atheromatous and fatty degeneration, with the numerous dangerous diseases they entail, we may be at a loss to account for the mortality. If we think, however, of smallpox mortality, 1 in 3 from the unmodified form, part of the difficulty vanishes, and then add malarial diseases, remittent fever, ague, diseases of the spleen and liver, anæmia and its consequences, and to these add cholera and leprosy, and we have sufficient causes for a considerable mortality. Another writes (Mr. Porter Smith, of Hankow)—"Chinese utter disregard of sanitary science among an otherwise highly civilised and accomplished race is one of the worst facts. They take kindly to the purple, but never to the

fine linen. The cleanest and whitest garment next the skin would be a strange perversion of the order of wearing their apparel, but nevertheless they possess public baths."

Dr. Somerville, of Foochow, a graduate of our University, says:—"I have to add my testimony as to the total disregard of anything like sanitary arrangements. Dirt of all kinds finds its way through the non-dovetailed and shrunken planks of the floor, and when a house is burnt or blown down, the foundation is seen to be a mass of filth in a decomposing state. There is nothing like drainage, and the traffic in night soil, the formation of manure pits, and the watering of fields with liquid ordure obtain here as elsewhere. In short, we have all the generally recognised factors of zymotic disease, with a high temperature to favour the fermentative and putrefactive processes. Yet we enjoy a high standard of health, and there has been no epidemic affecting foreigners at this port for at least eleven years." He was led to make these remarks by the remarkable fact, that four cases of typhoid fever occurred during 1872-73. There had been no case at the anchorage for three years, and in eleven years' practice there he had seen only seven or eight cases altogether before the present series. There was no evidence that the fever had been communicated from one patient to another, and he sets down the form therefore as sporadic. He adds, "We have a mass of evidence favouring the view that the poison of typhoid is communicated through contaminated water, and I think that cases like the present, where no such mode of diffusion is probable, are deserving of record." And he further adds, very cautiously, as if he felt misgivings about the received connection between contagion and the communication of typhoid, as explaining the whole case: "I think the purpose of these reports (Customs Half-Yearly Medical Reports) for the present is best served by collecting material for future use. I, therefore, content myself with stating these facts, and refrain from generalising from them, more especially as the subject of sanitation is at this moment engaging the attention of our best authorities in all parts of the world."

The foregoing remarks hold good regarding all large native cities. Drains for surface water are general in Chinese towns, but, for the most part, they have been allowed to get choked up and broken down, and then they become the receptacles, as open ditches, of the city garbage, and filth, and become a terrible nuisance. The condition of the capital resembles, in many respects, other Chinese cities, but in many important particulars it is quite different. In regard to drains, Pekin stands unrivalled among the cities of the world as far as their age, extent, former admirable adaptation and present ruinous

condition are concerned. We have two large sewers on all the main streets, with branches to all the lanes. They drain into the city moat, and these into the Peiho, or canal leading thereto. Those that are free in parts of their course are sure, after heavy rains, to open somewhere into the streets, deluging them with putrid mud and filth, entering the drain at a distant point, and then again repeating the above act. By and bye the mud dries, and is used for repairing the roads. With all our filth and dirt there is a wonderful immunity, even from fevers. The police, who water our streets from these cesspools, and utilise the dirty water and urine collected in the houses and shops during the day, are among the most robust and healthy of our population, provided they are not, at the same time, opium smokers. The beggars, a very numerous class, sleep in the streets nearly all the year round, congregate in the very centres of pollution, and even, to some extent, contest with dogs, priority of claim to the refuse of the dunghill, not only survive, but flourish, and most of them look fat and sleek. The mouths of our lanes, waste places, and tumble-down, or unoccupied houses and shops are our most polluted regions, and the common place of resort of the neighbourhood for males. The reason assigned for the innocuousness of this sad condition of things in China are, among others, the following: —We have our prevailing winds in most parts of China, which act as diluents, and prevent the too great accumulation of noxious effluvia, ready to ripen into activity. There is also the sandy, absorbent nature of our soil, in the North at all events. The apparently objectionable and disagreeable plan of watering the streets from the foul fluids of the drains and domestic urinals carries good in it. These collections of decaying organic matter are moved and prevented from accumulating in too large quantities, and when thrown on the streets the greater part of it is absorbed by the dry soil. The operation is always performed after sundown, to prevent too much evaporation. I am unable to say whether this is from economical or sanitary considerations, most probably the former. After sunrise the streets are nearly as dry as before. Our weather is very dry in the North, frequently seven or eight months without either rain or snow. In summer, after heavy drenching rains of several inches, the dust will be plentiful on the third day, so dry and absorbent is the soil. Another reason, and this is doubtless the chief one, is the high value of human manure, and the assiduity therefore with which it is collected for agricultural purposes. Large numbers of the people in this way obtain a living. Its high value and the great poverty of the people are our safeguards. It is carried outside the city walls, and is dried and caked, or pulverised. The dung of

the herbivorous domestic animals is dried for argol, and used as fuel. Human excrement so prepared is the most expensive of manures. Much that is recommended at home in the way of ventilation, water supply, and disinfection of privies is in China rendered unnecessary. All the advantages claimed for the dry earth system are gained here free of expense to the individual or public. The industrious and frugal habits of the Chinese, and even their very poverty, thus work to their advantage (all sanitary measures more than repay their cost), for it compels them to utilise all excrementitious matter. Every particle of every kind of manure, besides rags, paper, &c., are collected and preserved with the greatest care. The private privies, which are all out of doors, are visited daily by these manure collectors, and so great is the demand for it, that no payment is made to these scavengers. Foreigners pay a trifle monthly to guarantee respectability, cleanliness, and regularity on the part of the collector. The healthiness of our foreign settlements in China is, in a great measure, owing to the absence of water closets in the dwelling-houses, which, in Europe, are a fruitful source of disease. Gases, such as sulphuretted and carbonetted hydrogen, are not so injurious to health when given off in the open air, as when escaping from sewers. China is, par excellence, the country of bad smells, and yet, as we have seen, the people do not seem to suffer from them, but, on the contrary, rather like them.

The removal of excreta and the disposal of sewer water is the sanitary problem of the day in this country. Our sewers allow transference of gases and organic molecules from house to house and place to place; occasionally, by bursting, leakage, or absorption, the ground is contaminated, and the water supply is constantly in danger of being poisoned and contaminated; and all these dangers are greater from being concealed and being beyond individual control. Fevers and cholera are thus possibly propagated from house to house. In China we are entirely free from this danger. It would seem advisable that our water closets should be in an out-building or projection from the main house, and should be placed at the top of the house, with a tube passing to the outer air. When placed in the basement the closet air is certain to be drawn into the house. For use in the bath room, a solution of sulphate of iron 4 parts, carbolic acid, 1 part to 30 parts of water, has been recommended as a good disinfectant by Dr. Jamieson. Each time it is used two ounces of the solution should be poured in. By this means the air would be rendered perfectly pure, or at most faintly impregnated with carbolic acid. The great expense attending the use even of the cheapest disinfectant would seem to preclude the use of this method for the large

sewers of our cities. This, however, is denied, and it is asserted that certain antiseptic agents, even in a highly diluted form, would gain the object in view. It does not seem impossible in large manufacturing towns to ventilate the sewers by connecting them with factory chimneys. These same chimneys, as well, probably, as those of ordinary houses, in seasons of epidemics at least, or by spray-producing machines or other mechanical appliances, might disinfect closes, houses, and whole localities and towns.

The water supply in China is derived chiefly from rivers and wells. The water obtained from rivers is manipulated in various ways, passed through filters, allowed to settle, and the organic matter is precipitated by chemical agents, alum being in most common use. But whatever be the water, and wheresoever the supply, it is invariably boiled by the Chinese. The Chinese, as a rule, do not drink cold water. We have already pointed out how strongly it is contaminated with filth and garbage in the great centres of population on the great rivers, and that the supply is almost invariably drawn from the public places of resort.—*Glasgow Medical Journal, April* 1877, *p.* 179.

9.—REMARKS ON ARTERIO-CAPILLARY FIBROSIS AND ITS CLINICAL RECOGNITION.

By Dr. F. A. MAHOMED, Pathologist to St. Mary's Hospital.

It is a matter for regret that the discussion on the important question of arterio-capillary fibrosis that has been brought before the Society in so exhaustive a manner, as far as concerns its pathology, by Sir William Gull and Dr. Sutton should be limited so entirely to the consideration of the comparatively trifling question of whether the undoubted thickening of the arteries seen in these cases be due to muscular hypertrophy or fibro-hyaline change. Both probably are present. If any other question has been raised, it is of almost equal insignificance —namely, whether the also undoubted increase of fibrous tissue and decrease of gland tubes in the contracted kidney be due to compressed, shrivelled, and atrophied tubules, or to a new growth of intertubular fibroid tissue. For my own part I believe it to be due to the latter cause, and I think I have seen it in every stage, from a highly nucleated intertubular exudation in acute disease, passing through every phase of organisation till it reaches the dense, old, and sparsely nucleated fibroid tissue seen in the typically small contracted kidney.

But the question raised by Sir William Gull and Dr. Sutton seems to me of a much larger nature: it is whether or not there is a great and general "fibroid degeneration" occurring in many

persons after, or even before, middle life, and which they call "arterio-capillary fibrosis." This question is by no means a new one; it was long ago raised by Sir William Jenner and by Dr. Handfield Jones in their papers in the Transactions of the Royal Medico-Chirurgical Society and in the Medico-Chirurgical Quarterly Review respectively. Those papers raise the great question whether there is not such a disease as a general fibroid degeneration which may be classed with other general degenerations, such as the fatty and the amyloid; I think, moreover, they prove it to exist, and arterio-capillary fibrosis appears to be nothing else. The fact that this disease does exist as a great pathological entity has almost been lost sight of, and yet I think most pathologists would recognise the fact that such cases often occur, and their pathology is well known in detail, although not always recognised as a whole.

Cases are familiar enough to me which I should call, following the teaching of Dr. Handfield Jones, cases of "fibroid degeneration," in which one finds a general increase of fibroid tissue in all the organs. They have firm brains and spinal cords, with thickened and opaque arachnoids, and much subarachnoid fluid; ossified cartilages; tough and fibrous lungs, with prominent bronchi; more or less cirrhosed livers, or rather livers with an excess of fibrous tissue; contracted and firm spleens; more or less granular kidneys, with adherent capsules; the mucous membrane of the alimentary canal presenting the usual appearances of chronic gastro-intestinal catarrh; the heart has commonly a hypertrophied left and dilated right ventricles, with thickened valves, containing some white fibroid or atheromatous patches; a more or less atheromatous aorta and larger vessels, and thickened smaller ones: shortly, in fact, an increase of the fibroid tissue throughout their bodies. Such, roughly, are the microscopic appearances of "fibroid degeneration," and such, I take it, are those of arterio-capillary fibrosis.

The modes of death in these cases are various. They may simply die of exhaustion, or "general decay," no especial symptoms predominating; in others the kidney symptoms will be more marked, and they may be classed as chronic or acute Bright's disease, and die from any of the ordinary complications of this disorder; in some, the lungs may suffer most, and bronchitis or pneumonia terminate life; occasionally the symptoms arising from the liver may be aggravated, and death may appear due to cirrhosis. Apoplexy, aneurism, and symptoms of heart disease are the necessary results of these cardio-vascular changes, and are frequently the immediate causes of death; any fever or acute disease puts them in imminent peril. Such cases may be found therefore scattered throughout the post-mortem records under various headings, according to the complication

under which they finally succumbed; but this general condition of "fibroid degeneration" was the ultimate, though not the proximate, cause of death.

Sir William Gull and Dr. Sutton have stated in their paper that, though they have now studied fully its pathological results, they cannot with certainty recognise its clinical indications. Although in the presence of this statement it would appear absurd and presumptuous were I to pretend to do so, still I cannot but think that the sphygmograph has afforded the key to its recognition. It is very common to meet with people, apparently in good health, who have no albumen in their urine or any other sign of organic disease, but who constantly present a condition of high arterial tension when examined by aid of the sphygmograph. I pointed out the common occurrence of this condition in a paper on the "Etiology of Bright's Disease" published in the Transactions of the Royal Medical and Chirurgical Society for 1874, and I suggested that they were the subjects of arterio-capillary fibrosis.

Such people, however, are very commonly subjects of a gouty diathesis, dyspeptics, suffer from functional derangements of the liver, indulge too freely in alcohol, or have, from one cause or another, tainted or impure blood. It does not require the sphygmograph to recognise this most important character of the pulse. Among the characteristics it presents to the finger that of "persistence" is the most important—namely, the ability to feel the artery constantly distended during both the systolic and diastolic period; it is always present as a more or less rigid cord under the finger—rigid, however, not from thickening or disease of its coats so much as from its constant hyperdistension. Now the pathological results of this condition of high arterial tension are most important and most evident. Given that a certain thickness of vessel is required to withstand the distending power of a normal amount of arterial pressure, it must necessarily follow, if that arterial pressure be very greatly increased, that the vessels must be correspondingly thickened to prevent their overdistension. So with the heart, which must also become hypertrophied to overcome the increased arterial resistance. Thus do we see the cause and the necessary production of the characteristic cardio-vascular changes in arterio-capillary fibrosis. Should this condition of hyperdistension extend to, and originate in, the capillaries, as I believe it does, it should produce the changes invariably subsequent to chronic congestions—namely, increase of fibroid tissue. There is good reason to think, therefore, that a constant condition of high arterial tension must of necessity produce an arterio-capillary fibrosis.

It may be suggested that the anatomical changes first occur, and that the increased arterial tension is merely a result, and

not a cause; but I have demonstrated, in the paper previously referred to, that this increase of arterial tension may be a very temporary condition, or may exist for any length of time or throughout life. It is found after scarlatina previous to and during albuminuria, when it may last for only a few days or even a few hours; it occurs more gradually and for a longer period in pregnancy; it may be produced by nervous causes, such as troubles and anxieties, and pass away with them; or it may occur still more insidiously, and last till death, in chronic Bright's disease.

The causes of high arterial tension may be temporary or permanent. They are usually some form or other of blood-poisoning—namely, gout, lead, alcohol, pregnancy, constipation, scarlatina, and certain forms of dyspepsia, mal-assimilation, and functional disorders of the liver; to these blood conditions certain mental causes may be added. Though generally present in both acute and chronic Bright's disease, the increased arterial tension does not appear to be due to the kidney disease itself, but rather to the preceding blood condition which gave rise to it. Proof of this is seen in the fact that during the recovery from acute Bright's disease, while the urine is still highly albuminous and full of granular and fatty casts, so that the kidney is manifestly crippled and unable properly to discharge its functions, the tension often disappears from the pulse. This is a most important point in prognosis; it indicates that the blood condition which caused the disease has passed off, and the patient will probably make a complete recovery; while those cases in which the tension remains high, while the dropsy and albuminuria disappear, will probably pass on to chronic Bright's disease and contracted kidney, the blood-poison being of a chronic nature or due to a constitutional condition.

Again, patients suffering from chronic Bright's disease do not invariably present high arterial tension during life, or a hypertrophied heart after death.

Some cases have been referred to by Dr. Gowers in which, after destruction of one kidney by disease, the arterial tension has been increased and the heart hypertrophied; but most probably this increased arterial tension existed before the destruction of the kidney, being possibly due to a gouty diathesis, which had also caused the formation of a calculus in the kidney, giving rise to its subsequent suppuration and destruction.

From the foregoing remarks, I think the following propositions may be deduced:—

1. Certain temporary or permanent blood-poisons will produce temporary or permanent increase of arterial tension.

2. Persistent increase of arterial tension will necessarily

produce the cardio-vascular changes seen in arterio-capillary fibrosis.

3. Blood-poisons, giving rise to increased arterial tension, are commonly, but not invariably, the causes of acute and chronic Bright's disease.

4. High tension may exist without kidney disease, though rarely.

5. Kidney disease may exist without high tension, but also rarely.

6. That high tension is the result of an antecedent blood condition, and not of the subsequent kidney disease.

Finally, without entering into the discussion of the theory of "stop-cock action" of the muscular coat of the arterioles, I would point out the importance of the observation by Sir William Gull and Dr. Sutton of what they describe as a hyaline exudation from the walls of the *capillaries*, causing thickening of them and in growths of fibroid material into the surrounding tissues. Taking into consideration the existence of high arterial tension preceding and causing the changes in the vessels, it appears that these changes around and in the capillaries are the results of high pressure in the capillaries themselves. They are the results of chronic capillary congestion, while dropsy and albuminuria are the results of acute capillary congestion. Surely these could hardly occur if the stop-cock action really existed, for this would necessarily protect the capillaries from the results of high tension, which would not reach them. These observations appear to me somewhat antagonistic to Dr. Johnson's theory.—*Lancet, August* 18, 1877, *p.* 232.

10.—A NEW METHOD FOR THE QUANTITATIVE DETERMINATION OF SUGAR IN BLOOD.

By Dr. F. W. Pavy, F.R.S.

[The following is an account of Dr. Pavy's new gravimetric process for determining the quantity of sugar in blood. He adopts the use of the galvanic battery for effecting the deposition of copper which has been reduced by the sugar in a form to be susceptible of weighing.]

A certain volume of blood—about 20 c.c. forms a convenient quantity—is taken for analysis, and first mixed with 40 grammes of sulphate of soda; the whole must be subjected to weighing in detail, so that the precise weight of the blood taken may be known. To this mixture, contained in a beaker of about 200 c.c. capacity, about 30 c.c. of hot concentrated solution of sulphate of soda are added, and the whole contents heated until a coagulum is formed.

Filtration is then performed, and the coagulum thoroughly

washed, so that all traces of sugar may be removed. The liquid thus obtained, from having been run and squeezed through muslin, is slightly turbid, and must be boiled again and filtered through paper to render it perfectly clear. It is now ready for the application of the copper test. Being brought to a state of ebullition, about 10 c.c. of the potassio-tartrate of copper solution, or sufficient to secure that the test liquid is left in excess, are added, and brisk boiling continued for a minute, but not longer. In this way a reduction of the oxide to the suboxide of copper is effected by the action of the sugar present in the solution.

The liquid is then filtered through a plug of asbestos, or, what is better, glass wool. The suboxide, having been collected and washed from excess of the copper test liquid, is next dissolved by a few drops of nitric acid, a small quantity of peroxide of hydrogen having been previously added in order to effect oxidation and consequent ready solution.

The copper present in the liquid is now deposited by the agency of galvanism. The positive pole of the battery is formed by a platinum spiral coil, round which and forming the negative pole is a cylinder of platinum foil; upon this the copper is slowly deposited in a pure metallic form. This operation is continued until the appropriate test shows that the whole of the copper has been thrown down. The period ordinarily required to effect this does not exceed twenty-four hours.

The platinum cylinder is next removed, and instantly plunged first into distilled water, and then into alcohol. After drying in a water oven it is ready for weighing; the difference in the weight of the cylinder before and after the operation gives the amount of copper deposited.

The battery used is a modification of Fuller's mercury bichromate battery, and has been selected on account of the constancy of its action.

From the amount of copper deposited, that of the sugar existing in the blood analysed may be accurately calculated. Five atoms of the cupric oxide of the test solution are reduced by 1 atom of glucose; it follows that 317 parts of copper represent the equivalent of one part of glucose, or the relation stands as 1 of copper to 0·5678 of glucose. Therefore, to ascertain the amount of sugar, the weight of the copper has to be multiplied by 0·5678. This application of the copper test solution yields a gravimetric instead of a volumetric process of analysis, and one which has no uncertainty belonging to it. There is nothing for the mind to decide, and no opportunity for error of judgment, as may be the case to a slight extent where a gradual fading of colour—as in the volumetric pro-

cess—has to be watched until the attainment of a proper point of the decolouration has been effected.

The accuracy and reliability of the foregoing process are strongly supported by the uniformity in the results obtained from a large number of experiments. Compared with the results yielded by this gravimetric process, those obtained by Bernard present the greatest discordancy. The figures he gives are invariably too high, and there is no intelligible relation in the differences noticeable, suggesting that there is something radically wrong in taking decolouration without precipitation of suboxide as a means of estimating the amount of sugar. Dr. Pavy supports this assertion by the conclusions derived from a large number of experiments.—*Lancet, July* 7, 1877, *p.* 13.

11.—ON THE TREATMENT OF RHEUMATIC FEVER.

By Dr. Julius Pollock, Senior Physician to Charing-Cross Hospital.

The treatment of rheumatic fever has lately undergone a complete revolution, which has happily placed it on a much more satisfactory footing. But a short time ago, a tolerably severe case was pretty sure to last six or seven weeks, almost uninfluenced by the remedies employed. Some put their faith in quinine, some in alkalies, some in various drugs, and some did nothing, with much the same result; and Sir William Jenner himself, when president of the Clinical Society, spoke of the doubt and uncertainty with which he used to approach the treatment of articular rheumatism under the old *régime.* It has been claimed for the alkaline treatment that it diminished the liability to heart mischief; but about this I think there is some doubt. If, however, it is thought desirable to try it, thirty grains of the bicarbonate of potash may be given every four hours, with or without five grains of nitrate of potash, in some peppermint-water or any other suitable vehicle. The potash produces no disagreeable effects, and may be continued for any length of time. It always diminishes the acidity of the urine, and sometimes makes it neutral, or even alkaline. With this internal treatment, the affected joints may be kept wrapped up in lint soaked in an alkaline lotion (bicarbonate of soda, one ounce; distilled water, one pint), and covered first with oiled silk, and then flannel or cotton-wool. In all cases of rheumatic fever the bowels should be kept gently open, but it is needless and undesirable to purge for mere purging's sake. The diet should be light, consisting chiefly of slops. Stimulants are not necessary as a matter of course; and the patient must remain quietly in bed until such time as his disease takes its

departure, which will vary, under this treatment, according to the severity of the symptoms or the tendency to relapse, from three to six or seven weeks, or even longer.

My late colleague, Dr. Hyde Salter, was in the habit of using quinine in the treatment of acute or subacute rheumatism, and I have had many opportunities of observing the result, which I cannot say impressed me at all favourably. Dr. Garrod combines the quinine and alkaline treatment, using a mixture made by rubbing up the quinine with the bicarbonate of potash, a little mucilage, and some aromatic tincture, in such proportions that each ounce and a half of the mixture contains five grains of quinine (in the form of carbonate) and thirty grains of bicarbonate of potash. This dose is given to an adult every four hours, and continued as long as may be deemed desirable. Dr. Garrod speaks favourably of this combination.

Of the treatment of rheumatic fever by bleeding, mercury, colchicum, antimony, it is unnecessary to say more than that modern experience has found such agents powerful only for evil. Iodide of potassium has been a good deal used, and though of but little service during the height of the disorder, it is often useful later on, helping us to "speed the going guest." Guaiacum is another drug which is sometimes successful in relieving the pain of the joints in the more chronic forms of articular rheumatism.

Reference must be made to the external modes of treating or assisting the treatment of rheumatic fever. Of these the chief are the hot-air bath, the application of alkaline lotion, cotton-wool, blisters, or iodine paint, to the inflamed joints. The hot-air bath has seemed, in some instances, to relieve pain, and its diaphoretic effects may be of service in eliminating the morbid material of the disease; but in a complaint like acute rheumatism, where the temperature is liable to range high, the application of external heat cannot be made without some risk, and the permanent benefit would appear to be doubtful. Besides which the excessive pain that attends any movement in the height of the disease would make it difficult, if not dangerous, to apply the remedy. The application of warmth to the affected joints is always grateful to the patient, and wrapping them up in cotton-wool or flannel generally alleviates the pain. The use of the alkaline lotion may prove beneficial, either in the same way or from some soothing influence connected with the alkali. Blisters and iodine paint are scarcely applicable during the acute stage of the disease, but are often of service subsequently by hastening the absorption of any fluid that may linger in the joints, and toning up the weakened parts. Blisters should be applied a little above the affected joint rather than over it; whilst iodine paint should be used cautiously, as, in

certain persons, it produces such an inflammation of the skin as to amount almost to erysipelas.

Such, then, was the more or less unsatisfactory state of things with regard to the treatment of articular rheumatism until within the last year or two, when Dr. Maclagan struck the keynote to a better mode of action by his researches into the use of salicin. This physician published a paper in the Lancet, "On the Treatment of Acute Rheumatism by Salicin," (*Retrospect*, vol. lxxiii., p. 34), from which it appeared that having been struck by some analogy between that disease and intermittent fever, Dr. Maclagan conceived that acute rheumatism might be of malarious origin, and receive benefit from the alkaloid derived from the willow bark. Without entering into any discussion of the theory which led to the experiments, there is no doubt that they were more or less successful, Dr. Maclagan detailing several cases of true rheumatic fever which, under the use of salicin, became convalescent, on an average, in four days. The first case treated was in November, 1874, and there is no doubt that Dr. Maclagan was the first person who drew attention to the value of salicin in rheumatism. Subsequently to the publication of the paper in the Lancet, large numbers of cases of the disease were treated with salicin, but with somewhat varying results, and in my own case, I confess, without any success. The dose given was generally from twenty to thirty grains, or more, every two, three, or four hours, and large amounts were required to be taken before much benefit was obtained. Such was the demand for salicin that the price of the drug rose from 1s. 6d. to 10s. or 12s. an ounce; and at one time there was an absolute famine, and wholesale dealers would quote no price for it.

In the mean time German physicians had been trying the effect of the derivatives of salicin—salicylic acid and the salicylate of soda. The second number of the Lancet of January, 1876, contained a notice of the observations of Dr. Reiss, in the Berlin Metropolitan Hospital, on the use of salicylate of soda, chiefly in regard to its action in reducing abnormal temperatures. Now, although salicylic acid and its soda salt may be valuable antipyretic agents in many cases of high temperature, independently of the nature of the disease, it soon became apparent that their good effects were especially marked in rheumatism. This led to the use of the drug in ordinary cases of rheumatic fever, and with the most satisfactory results. Some observers preferred the acid, some the soda salt. It is probable that the salicylic acid is the active agent in either case, just as the iodine is the active agent in iodide of potassium; but crude iodine is rarely given now, and in a short time I believe the salicylate of soda will be used in all cases where the

action of salicylic acid is desired. It is very soluble, which the acid is not, and it is far less liable to give rise to unpleasant symptoms. I give the preference most decidedly to the soda salt as at present advised, though it is quite possible, indeed likely, that combinations of salicylic acid with potash, ammonia, and iron, may turn out to be very valuable. In any case of articular rheumatism, whether acute, subacute, or chronic, the salicylate of soda should be tried in doses of ten, fifteen, or twenty grains, every two, three, or four hours, according to the severity of the symptoms. It is best to give it alone, or in combination with a little spirits of chloroform or syrup of orange. As a rule, the good effects of the drug are apparent after eight or ten doses; the temperature falls rapidly to normal, or even a little below, the pain and swelling of the joints disappear, and the patient is practically convalescent in two or three days; but it is better to keep up the action of the medicine for a week or so, as relapses are liable to occur if it be discontinued too soon. In some intensely rheumatic subjects it will be necessary to give it again and again before the disease is subdued, and these cases have been used as an argument against its efficacy. Some persons will not admit the value of mercury and iodide of potassium in the treatment of syphilis, and others question the protective power of vaccination against small-pox. All new remedies have to encounter the opposition of ignorance and prejudice, but the evidence in favour of salicylate of soda in the treatment of articular rheumatism is becoming so overwhelming that its great value must shortly be thoroughly established.

No doubt the drug every now and then produces disagreeable symptoms—sickness, deafness, tinnitus aurium, and sometimes a peculiar cerebral disturbance; but these quickly vanish on a discontinuance of the medicine, which may usually be again given in a short time without any such result. In the earlier trials, when the salicylate was not quite pure, these objectionable symptoms were much more common than now. Dr. Murchison has suggested, in an able paper read before the Clinical Society on the 25th of last May, that the disagreeable effects of the remedy are due to suppression of the function of the kidneys, and has found albumen in the urine of patients who were taking the salicylate of soda, even when the drug was quite pure. This may be so, but at present I have been unable to collect any evidence on the subject.

One word in conclusion. On its first introduction, salicylate of soda was thought to be of *special* value in the hyperpyrexia of acute rheumatism, but about this there is, I think, some doubt. It controls the temperature by counteracting the rheumatic poison, but in those cases which I have spoken of

early in this paper as malignant, it frequently fails to reduce the temperature, and is as ineffectual to cure the patient as large doses of quinine or the cold bath.—*Lancet, Oct.* 20, 1877, *p.* 564.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON THE STATE OF THERAPEUTICS IN TETANUS.

By the Editor of the Practitioner.

If tetanus were curable by chloroform we should all know it: its general use has ensured it a wide trial, and from the ease and quickness with which it can be introduced into the system, its effects are at first sight most brilliant, nor need it always, in order to relieve the muscular contraction and spasm, be pushed to narcotism. Under its influence the respiration becomes regular, the pulse infrequent, soft, and full, and this without giddiness, headache, or distress: it is even possible to feed the patient. Thus, Simpson narcotised a child for thirteen consecutive days, using ℥100 with success. But the general result is, that while all the fatal symptoms disappear upon the inhalation of chloroform, they return at its removal with unabated violence, and the disease generally comes then to its fatal conclusion without delay. A very good illustration of this may be found in a case reported by Dr. Panthel of Limburg, who used on the first day ℥vj. of chloroform without inconvenience, and yet after the most complete remission of all symptons, in the end lost his patient, who died with a small, hard, and rapid pulse, inhaling chloroform and conscious.

Chloroform is now very rarely administered in tetanus, but chloral hydrate and calabar bean, the drugs which most nearly approach it in respect of physiological action, are much in favour. Innumerable cases of the action of chloral are reported. To make any digest of them would be impossible, but in reading them one becomes convinced of two facts:—the first, that large doses must be given, and may be given without fear; the second, that such a treatment is most valuable. In the Progrès Méd. of July, 1875, there is a successful case reported in which the character of the disease was almost masked by the treatment, which was of a decidedly heroic nature, the daily dose of chloral being 155 grains, and intoxication being well marked. In the same paper reference is made to cases of death from overdose of chloral, which however are declared to be rare, and as an indication of the quantity that may be given the case is quoted of a child of twelve and a half years who took more than 200 grains a day. Dr. Ballantyne, of Dalkeith, gave ℥iij. in twenty-four hours, and ℥vj. ss. in three weeks with success; the

patient during this time being easily aroused to speak. Other and similar cases are well known; unsuccessful ones will be found in Bull. Gén. de Thérap., June, 1874, and in 1875 Alphonse Deu in a traumatic case gave as much as 150 grains a day without good result.

Calabar bean, which, like chloral, affects the spinal cord, and has a little or no action on the motor and sensory nerves, has also, like chloral, a high and well-deserved reputation. Eilert in 1873 (Zur Frage von der Behandlung des Wundstarrkrampfes) pretends that of opium, chloroform, chloral, curare and Calabar bean, the latter is the only one that acts satisfactorily on the spinal cord. He recommends either previous narcotism by chloral, or the simultaneous administration of atropin, so that both of these combinations have probably been tried. Already in 1864 Holthouse had reported two cases in the Lancet, one successful; he used three grains of the extract every two hours, and once 4½ grains in one dose. Maunder added two unsuccessful cases, and in 1867 Dr. Eben Watson reported two traumatic cases, which recovered, in the Edin. Med. Journ.; he had given by accident nine grains in as many hours. He also narrates his experience of the bean in Calabar, and gives ten experiments on animals; later he reported six of eight cases successful. Dr. Fraser in 1868 (Practitioner), urging the trial of the bean, which has since been largely used, gives a history of its use, with a list of cases. But Mr. Ashdown (Brit. Med. Journ.) in the same year had a case in which the drug seemed to fail, and Professor Spence, in the Lancet of January, 1875, reported another unsuccessful case, in which a boy of eighteen took twenty-two grains of the extract in 3½ days, and died on the fourth day. In last year's Lancet there are three cases by Dr. Dickenson, one of which died after having had about seven grains of the extract injected subcutaneously in the course of an afternoon without cessation of the spasms being procured: while of the two successful cases one was of an idiopathic, and the other a protracted nature.

The effect of the bean seems to be all that one would expect from its known properties; and that it is indicated the large doses requisite to obtain its physiological action seem to prove. The spasms are controlled and the body heat sinks, and if the drug be withheld the paroxysms return, while if it be pressed the patient comes into a somewhat dangerous condition. Those who have used it recommend its hypodermic injection, not less than ⅓ grain of the extract every two hours, and so large a dose is required to procure contraction of the pupil that it has even been seriously contended that its effect in subcutaneous injection is to dilate it. From the reported

cases it seems as if the first injection of Calabar bean or of curare sometimes produced a spasm.

Nitrite of amyl, which, like the three first-mentioned drugs, lessens the reflex action of the spinal cord, and has no action on the motor and sensory nerves, but, unlike them, excites the circulation, has been tried and failed.—*Practitioner, Aug.* 1877, *p.* 115.

13.—ON THE RELATION BETWEEN ANGINA PECTORIS AND PERIPHERAL ARTERIAL CONTRACTION; AND ON THE MODUS OPERANDI OF NITRITE OF AMYL AS A REMEDY FOR THE DISEASE.

By Dr. GEORGE JOHNSON, F.R.S., Professor of Clinical Medicine, and Senior Physician to King's College Hospital, &c.

We are indebted to Dr. Lauder Brunton for a knowledge of the fact that, in some of the most severe and alarming attacks of angina pectoris, the pain and the distress are speedily removed by inhaling the vapour of nitrite of amyl. Dr. Brunton was induced to employ this remedy by a process of reasoning based upon some previously ascertained data. He made repeated sphygmographic observations on a case of angina pectoris, and he noticed that, during the paroxysms of pain, there was evidence of high arterial pressure, the result of excessive contraction of the arterioles. The pain and the high arterial pressure coincided, and the pressure fell with the cessation of the pain. He, therefore, inferred that the increased arterial pressure was the cause of the pain. He says (Goulstonian Lectures, British Medical Journal, March 31st, 1877, p. 380. See also his paper in the Clinical Society's Transactions, vol. iii., p. 191), "The pathology of the disease thus seemed clear; and the next question was, how to treat it. The remedy wanted was one which would dilate the vessels, and this the researches of Richardson and Gamgee supplied. Nitrite of amyl they had shown to possess the very power which I desired, and thus their experiments on the pharmacology of the drug, and my observations on the pathology of the disease united, led to successful therapeutics. I administered the remedy, and the pain disappeared."

The influence of nitrite of amyl in subduing the pain and shortening the paroxysm of angina has been confirmed by a considerable number of observers. In two cases under my own care it has been remarkably successful; but, notwithstanding the success of Dr. Brunton's therapeutics, I doubt the accuracy of his pathology, and I believe that the *modus operandi* of the remedy is different from that which he assumes it to be.

I will first state, as briefly as possible, my objections to his

theory, and I will then suggest a different explanation of the phenomena.

There is no proof that arterial contraction is present in all cases of angina pectoris. So far as I know, Dr. Brunton's case is the only one in which the results of sphygmographic observations during the paroxysm have been recorded. Even if it were shown that contraction of the arterioles and high arterial tension are constant phenomena, proof would still be wanting that they are the cause of the angina.

If high arterial tension, with resulting backward pressure of blood upon the heart, were the sole or the main cause of angina pectoris, such symptoms might be expected to occur during the early stage of nitrous oxide inhalation, when before the period of anæsthesia there is unquestionable evidence of high arterial tension. (See my first Lumleian Lecture, British Medical Journal, April 14th, p. 446.)

In most cases of true angina pectoris, there is organic disease of the walls, or of the valves, or of the coronary vessels of the heart. (See, as to this, Dr. Gairdner's able and exhaustive essay on Angina Pectoris in the fourth volume of Reynolds's System of Medicine.) Dr. Brunton's theory suggests no explanation of the arterial contraction, or of the relationship between the central structural changes and the peripheral vascular contraction. It seems more probable that the pain is in some way directly due to these organic changes in the heart itself, than that it is a secondary result of a peripheral arterial contraction occurring without obvious exciting cause.

Dr. Gairdner in his essay (p. 375) refers to a paper by Nothnagel, who had in part anticipated Dr. Brunton by giving particulars of five cases of so-called "angina pectoris vasomotoria." Dr. Gairdner points out that Nothnagel's cases differ from cases of true angina in some important particulars, especially in the absence of severe pain; in the transient, trivial, and curable character of the attacks; and, in the fact that, the sensations in the extremities supposed to result from vaso-motor spasm (deadness, coldness, formication and pain) were usually present in all the extremities indifferently, and *preceded* the palpitations and cardiac uneasiness by some minutes. And Dr. Gairdner remarks that, "the lesson taught by Nothnagel's cases is not, properly speaking, that typical, still less that fatal, angina pectoris, is always to be regarded as due to vaso-motor spasm, but rather that, under certain peculiar conditions of the system, a sudden check to the circulation in the extremities determined by vaso-motor spasm may become the cause of an increased action of the heart, palpitation, and pseudo-angina; the disease so induced, however, being devoid

of the characteristic pains, and the more aggravated phenomena of fatal angina."

I now pass on to my own explanation of the relationship between the pain of true angina pectoris and the arterial contraction; also of the *modus operandi* of the nitrite of amyl in these cases.

I believe that we have as yet no data which will enable us to give a complete explanation of the pain of angina pectoris, whether it be a pure neuralgia with consequent inhibition of the cardiac contractions, or a result of cardiac muscular cramp, or of overdistension of one or more of the heart's cavities, or a combination, in some cases at least, of two or more of these conditions, must remain at present a matter of doubt. It is agreed on all hands, that the pain is often agonising, and that it often radiates into neighbouring sensitive nerves, more especially those of one or both arms. It is a fact ascertained by experiment, that electrical irritation of the central end of a mixed or a sensitive nerve, such as, for instance, the sciatic or the trigeminal, not only causes severe pain, but also excites by a reflex influence through the vaso-motor centre, a general contraction of the arterioles, with resulting high arterial tension. (See Vulpian, Leçons sur l'Appareil Vaso-Moteur, tome i., p. 237, &c.) I, therefore, venture to suggest that the high arterial tension, when associated with angina pectoris, is a secondary reflex result, and not the primary cause of the cardiac agony; and, further, that the remedial efficacy of the nitrite of amyl is due to its remarkable influence over some forms of neuralgia, and not to its relaxing effect upon the arterioles, except in so far as its antineuralgic power may depend upon its influence on the arterioles. My contention is that, in addition to the centripetal nervous influence which causes the pain of angina, there is an influence reflected from the nervous centre through the vaso-motor nerves, and thus exciting a more or less general contraction of the arterioles. It is probable that the peripheral arterial resistance, although not the primary or the main cause of the pain, yet adds to the distress and the danger of the paroxysm. We have no evidence to prove that in any case of angina there is a general arterial contraction. It may be that the arterial contraction, like the pain, is limited to one or both arms.

A true theory of the relationship between angina and arterial contraction, and of the manner in which the nitrite of amyl affords the remarkable relief which it often does, is not without practical utility, more especially as a guide in the selection of cases which are suitable for the employment of the drug. One of the most striking examples of great and permanent relief by the use of nitrite of amyl is afforded by the case of

Dr. Herries Madden of Torquay, as recorded by himself. (Practitioner, 1872, vol. ix., p. 331 ; quoted also in Dr. Gairdner's essay, before referred to.) Dr. Madden states, that for a considerable time he had hesitated and neglected to employ the nitrite of amyl, in consequence of his belief that it was suitable only in those cases in which the face is pallid during the paroxysms. "As mine was flushed," he says, "I dismissed from my mind all thoughts of trying it, and paid the penalty of hasty conclusions in the shape of a large amount of acute suffering." The result of a first trial of five drops inhaled during a severe paroxysm "was truly wonderful. The spasm was, as it were, strangled in its birth ; it certainly did not last two minutes instead of the old weary *twenty*, and so it continued. The frequency of the paroxysms was not diminished for some time ; but then they were mere bagatelles as compared with their predecessors. Under these improved circumstances, strength gradually returned, the attacks became less and less frequent, and finally ceased." Now, in this case, although the relief from pain was associated with the usual evidence of the physiological action of the drug in relaxing the arterioles, the flushing of the face during the paroxysm forbids the conclusion that a general contraction of the arterioles was the cause of the cardiac distress, and that the relief was due to relaxation of the arterioles.

Dr. Talfourd Jones has given some good illustrations of the therapeutic value of the nitrite of amyl, not only in relaxing spasm, but also in rapidly relieving neuralgic pains. (Practitioner, 1871, vol. vii., p. 213.) Dr. Jones's experience has been confirmed by other clinical observers.

A few weeks since, I was consulted by a lady who for a fortnight had been suffering from severe facial neuralgia, which had resisted various remedies that had been employed before she came under my care. I advised her to drop five minims of nitrite of amyl on blotting-paper and to inhale the vapour. The pain was rapidly and completely subdued. In a few hours it returned in a milder degree, and it was again promptly removed by the amyl vapour, and, after three or four repetitions of the dose, the cure was complete. It would be interesting, and it might be instructive, to observe, whether during a severe attack of facial neuralgia or sciatica, there is any evidence of contraction of the arterioles, with resulting tension of the arteries.

In conclusion, I repeat the expression of my belief that the relief which is often afforded by nitrite of amyl during a paroxysm of angina pectoris, is due to its antineuralgic power, and not directly to its relaxing influence on the muscular arterioles.—*British Medical Journal, June* 23, 1877, *p.* 770.

14.—ON HYSTERIA AND ARREST OF CEREBRAL ACTION.

By Dr. SAMUEL WILKS, Physician to Guy's Hospital.

If two watches were lying on the table and motionless, it would be impossible to decide without any further knowledge which was quiet as a result of a broken spring and which as a result of not being wound up. It is the fact of these negative symptoms which makes the diseases of the nervous system so difficult to diagnose, there being scarcely a single organic disease which is not simulated by a functional one. It seems, indeed, to be absolutely true that the machinery of the nervous centres may actually cease working or functionising, and thus, like the unwound-up watch, be for all active purposes diseased or dead.

We have been in the habit of endeavouring to force all our cases into a given category of pathological changes, and then to speculate to which in the list any one of them appertains. And in this matter fashion to a certain extent guides us, for as formerly inflammations and congestions were regarded as the most probable cause of disturbance of the cerebro-spinal centres, so now, on the other hand, anæmia, brought about by the action of the vaso-motor nerves, has taken their place. There is no proof that either one or the other of these blood changes is instrumental in the production of conditions where the centres temporarily cease to act; nor is it likely that in any such aberrant affections implying rapid oscillations in the nerve elements any alterations could have occurred which are conceivable by the mind's microscopic eye. For my own part, I at present rest upon the fact that any part of the nerve centres may cease to work and be productive of the same symptoms as if an organic disease were present; also that such inaction may remain for an indefinite time, and may ultimately lead to changes which may be visible after death. Very often these temporarily dormant states may be rapidly recovered from unless death occur through the paralysing of some vital part, but then no visible disease is found in the nerve centres; nor is it likely that any of the ordinary degenerative processes would be discovered in them.

There is no disease of the cerebro-spinal centres having an organic cause which may not be simulated by a functional one. Even in the cases which recover, if we introduce such a cause as inflammation and the effects of pressure, our difficulty of understanding how loss of function occurs without destruction of tissue still remains. For example, every one must have seen in cases of fatal paraplegia resulting from caries of the spine a mass of lymph pressing on the cord, which therefore must be regarded as the cause of the paralysis in those cases which recover. Thus I have seen more than one patient lying in bed

for many months with angular curvature and complete paraplegia, accompanied by bedsore and constant need of catheterism, eventually get perfectly well; whence it follows that the new product surrounding and squeezing the cord must have arrested its action without in any way injuring its structure. In what manner it did this is not known. It might be by squeezing the tubules and deranging their elements, but whether this could have been made apparent to the eye is very uncertain. The nature of the difference between a cord in action and one at rest is as difficult to conceive as that between a simple wire and another in a state of electric tension; we only know they are not identically the same by the difference of the phenomena which they exhibit, and for the sake of an expression we style these unknown functional conditions dynamic.

In hysteria with profound torpor and an inability to move the limbs, together with an insensibility to every impression, the condition of the patient is not very different from that of sleep. In sleep volitionary power has gone, the limbs are flaccid, and receptivity from the outer world has ceased. In this natural period of existence the brain is supplied with a less amount of blood than when it is in full activity, and therefore it might be surmised that certain variations in the amount might be connected with the loss of function in the hysteric state; of this, however, there is no proof. I allude to it because no explanation has been offered of a sudden paraplegia, quickly recoverable or fatal, except by some diminution of blood supply through the action of the vaso-motor nerves. If this were the case the spinal cord would sleep, an idea quite in accord with the commonly expressed doctrine that the spinal system never sleeps, implying that sleep signifies paralysis and death. Because during the inactivity of the brain there is less blood, it does not follow that this diminution is the first stage towards sleep, as it may be a consequence only, more subtle causes being behind it. So in the spinal cord it may be that less blood is flowing through it in some forms of spinal paralysis, but this perhaps is only one of the phenomena of spinal sleep. Whether this be so or not, sleep of the cord would denote a condition of the direst import.

Now, it would appear that the higher ganglia or cerebral hemispheres may cease to act, as in sleep, and yet be structurally intact. It is in consequence of this that the difficulties mentioned in the cases of cerebritis arose, and patients with a universal disease of the hemispheres were thought to have no worse a malady than hysteria. The most intelligible and best known cause for a cessation of cerebral action is anæmia, or want of blood, as witnessed in the unconsciousness of sleep, of fainting, or in Sir Astley Cooper's experiments, where

insensibility was instantaneously produced by a ligature of the blood-vessels. There are other and unknown circumstances which also arrest the activity of the brain or paralyse it; one of these may be a violent moral shock, and as a consequence all the higher functions connected with the superior cerebral ganglia disappear, and the patient lies senseless, without volition, the animal and spinal part being alone in operation. This complete abeyance of the supreme functions of the nervous system is one of the most characteristic features of hysteria; so much so that where no one has yet succeeded in giving a definition of the disease, this inaction of the cerebral hemispheres leaving the spinal system to have its full play, gives as good and correct idea of its nature as any other definition. It is clear that the possessor of a very powerful will or authoritative disposition could not be hysterical except under the influence of a tremendous shock to the system, whilst on the other hand hysteria would be constantly found amongst the nervous and feeble-minded. It approaches therefore in many cases to actual insanity, which in itself is often nothing more than a loss of control over the lower animal instincts and passions; in fact, a definition of insanity has been founded on the loss of will power, and that it is rather this than an absence of intelligence which is so often seen in lunatics. It is exactly on this point that members of the medical and legal professions differ. Coleridge saw that the will could be separated from the understanding, for in allusion to his own melancholy addiction to opium eating, he says, "My case is a species of madness, only that it is a derangement, an utter impotence of volition, and not of the intellectual faculties." Certain it is that in many forms of hysteria the prominent condition is one in which the higher mental faculties have ceased to control the lower, and the spinal system is in full play. In hysterical attacks with convulsive movements one is reminded of the wriggling of an animal without a head; in other cases the patient lies in a state of torpor; all perception has gone as well as volition; if this condition becomes chronic, a state of trance ensues, in which the patient is brought to the lowest state of vitality, and resembles in many respects, and especially from the small amount of food which is required, a hybernating animal. We cannot therefore, but conclude that when an hysterical woman is anæsthetic and paraplegic, her brain has lost its activity. She does not feel, she does not move, the cerebral powers are dormant. If this be so, what other conclusion can be arrived at in cases of hemiplegia and hemianæsthesia of an hysterical or ideal nature than that one hemisphere has become inactive or ceased to work whilst the other is in full operation?

A fright will make a woman long bedridden leap from her

couch, the sounds of long-forgotten music will sometimes have the same effect, and, in fact, any circumstance which will act on her sensorium may afford the corrective stimulus. Moral treatment is the method eminently rational and successful, whilst that which is too often adopted is a system most perfectly devised to perpetuate the hysterical state. By regarding it as a positive disease and fostering this idea by constant medical attendance, by nursing and the administration of medicine, a mode is adopted exactly suited to convert a functional trouble into a real malady. The speedy cures which occur in hospitals amongst patients who have had nervous maladies of months' and even years' duration contrasts most remarkably with the failure of treatment in like cases in private practice.

The theory which ascribes the complete abeyance of the will and of sensation in hysteria to a cessation of the activity of the higher functions of the cerebral hemispheres is quite in accordance with the facts observed in this disease; if we also believe that during the absence of this governing power of the brain the spinal system is allowed to run riot, we can understand the meaning of the convulsions, strange movements, and emotional excesses so frequently witnessed in this malady.

Any part of the cerebro-spinal system may slumber, yet it is important as well as interesting, to observe other morbid states of the nervous system, which are often associated with this condition of torpor. I have already alluded to abnormal movements, and I may direct attention to hyperæsthesia and neuralgia. Where a constant movement exists we suppose that the motor centres are in a state of over-excitation; for example, if I imitate the movements of choreal patients I am exerting an influence on the corpus striatum, and therefore when the action is involuntary it is supposed by many that this ganglion is in a state of perpetual morbid irritation; so, again, if I imitate the tetanic state, I am throwing the motor columns momentarily into an excitation which they permanently assume in real tetanus; this state therefore being clearly a dynamic one. Also those lesser choreal movements which are so common mean nothing more than the perpetual activity of a motor centre, which should only naturally be excited under the power of the will. In some cases the strange movement is not morbid in any sense, it is simply purposeless; for since the grey centres are educated for special useful motions, which they come to perform without the guidance of the will, so they may contract after a time some habitual odd movement; but this cannot be regarded as a disease. If this morbid irritability of the motor centres be allowed, as well as their liability to continue in action beyond the control of the will, there is no difficulty in admitting

the possibility of an analogous condition in the sensory centres; if so, a hyperæsthesia would result. If, again, that condition into which the sensory centres are thrown when pain is produced should continue, a neuralgia or subjective pain would result. It is at present a problem beyond our solution to discover into what different state the higher nerve centres are thrown when, on touching the surface of the body, a common sensation or pain is experienced, but we can imagine that if a similar state of susceptibility arise from causes within the centres themselves, then an extreme sensibility or actual pain would be felt as a purely subjective symptom.

An extreme susceptibility of the nerve centres constitutes a form of ordinary neurosis we are daily meeting with, the subjects of it becoming a prey to hyperæsthesia and neuralgia. The whole question of pain from a clinical point of view is one of the greatest importance; indeed, a large part of one's daily occupation is the attempt to unravel the meaning of pain and discover whether it be really objective and have an actual cause in the painful spot or whether it be altogether subjective. As a general principle it may be said that the outside of the body or that which comes in contact with external objects is sensitive whilst the interior is devoid of sensation, that is, injuries to the exterior skin or mucous membrane cause exquisite pain, whilst incisions into the solid viscera or the stomach and bowels produce none; and the same is true of the bones and muscles. In disease, however, the case is different; in inflammation of the thoracic and abdominal walls acute pain is experienced, but here branches of the same nerves are involved which supply the skin without. In the case of the solid viscera there is little proof that disease of any kind is felt, but in the hollow organs, as the stomach, intestines, ducts, so also in the muscles, most exquisite pain is felt when a compression of the tissue takes place, or in spasm as it is called. All these painful states are positive and objective, but besides these there are pains which are subjective, or, at least, have not the usual origin, in an irritation of the periphery of the nerve; in many cases of this kind the nerve itself may be at fault, but of this there is no proof.

When we use the term neuralgia we generally imply, on the one hand, that there is an absence of any local cause to produce the pain, and, on the other, that it is not merely subjective, but that there is something abnormal in the nerve itself which is productive of the symptoms. In many cases it is difficult to decide between these three states, one having an ordinary local origin, another due to a morbid state of nerve, and a third a purely subjective condition arising from over-sensitiveness of the centres. It requires all the medical man's acumen to

unravel these several causes. The patient himself cannot distinguish between them, and from my own knowledge I am sure that no power of discrimination belongs to us; indeed, to the patient himself there is no difference between his having a pain in a part and thinking he has a pain in that part. In a state of health a man should have no knowledge of the working of his animal machinery, but as soon as he gets out of gear he becomes conscious of his head, his heart, his stomach, and other organs, and moreover he begins to experience aches and pains. He has headache, or backache, pain in the side, or pain in every part of the body. In the condition known as hysteria, there may arise neuralgia, myalgia, enteralgia, and every other algia. In various material diseases the same, however, may be seen, as in poisoning by lead and in alcoholism, where there is not a part of the body in which pain may not be experienced. In some instances of the kind there may be an actual change in the nerve, but in others the cause can only be attributed to a morbidly sensitive state of the centres.

We must conclude that in hysteria and some other conditions whose pathology is not known the cerebro-spinal centres may become dormant, and also that they may, on the other hand, become over-active, giving rise to increased motility or increased sensitiveness.—*Guy's Hospital Reports*, 1877, *p.* 26.

15.—ON THE NERVOUS SYMPTOMS RESULTING FROM URÆMIA.

By Dr. George Johnson, F.R.S., Senior Physician to King's College Hospital.

There is good reason to believe that some of the more formidable *nervous symptoms* which result from uræmia—in particular uræmic convulsions and a form of transient amaurosis—are directly due to cerebral anæmia consequent on sudden extreme contraction of the muscular arterioles.

There can, of course, be no question that uræmic convulsions are of an epileptic character. A large amount of evidence points to the conclusion that both the loss of consciousness and the convulsions of epilepsy are the results of sudden and extreme anæmia of the brain. In man, and in most, if not in all, warm-blooded animals, a rapid and very copious hemorrhage usually causes convulsions. Kussmaul and Tenner state (On the Nature and Origin of Epileptiform Convulsions caused by Profuse Bleeding, New Sydenham Society, 1859) that in numerous cases of dogs, cats, and rabbits, they observed, without a single exception, violent and general convulsions preceding death by loss of blood. In order to produce this result, the hemorrhage must be rapid. If it occur slowly, so that the vital powers are

gradually exhausted, death then occurs with swooning, drowsiness, and delirium without convulsions.

The same experimenters found that an interruption of the supply of blood to the head of a rabbit, by ligature or compression of the arteries of the neck, produces epileptic convulsions as surely as hemorrhage does. In about one hundred rabbits they ligatured or compressed the carotids and subclavians, from which, be it remembered the vertebral arteries proceed; and in every case, except that of one very old lean and feeble animal, convulsions occurred.

In order to excite convulsions, they found it necessary to close all the four arteries which supply the brain. If but one carotid or one vertebral artery remained pervious, the animal was enfeebled and more or less paralysed, but not convulsed. And again, if, during the height of the convulsion, the ligature were removed from one carotid, the convulsions generally ceased immediately, and there was a sudden change from the most frightful spasm to complete relaxation of the muscles. The description of the convulsions thus artficially produced with, as it seems to me, needless reiteration, in the lower animals, shows that they were essentially the same as epileptic convulsions in the human subject. There was the dilated pupil, the tonic spasm, quickly followed by clonic convulsion so violent as to throw the animal forward to a distance of one or two feet, and sometimes even over the shoulders of the operator. These experiments obviously could not be performed on the human subject; but Drs. Kussmaul and Tenner approached as near to this as they dared by compressing the carotids of six men. The result was that in all the face turned pale; the pupils first contracted and then dilated; the respiration became slow, deep, and sighing; then there was giddiness, staggering, and unconsciousness, and the men would have fallen if they had not been supported. They say that, "in two subjects of weak intellect and moderately anæmic, in whom, notwithstanding the above symptoms, the compression was continued, a choking sensation, attended by vomiting and general convulsions, came on, which, however, did not attain an aggravated form; for, on withholding the compression, they disappeared in a few seconds." (*Op. cit.*, page 28.) Compressing the carotids does not, of course, entirely cut off, but only greatly lessens the supply of arterial blood to the brain; but these experiments render it probable that sudden occlusion of all the arteries supplying the brain would as certainly excite epileptic convulsions in man as in the lower animals. And this conclusion is confirmed by observing the results of certain diseases and accidents in the human subject. Thus convulsions occur almost invariably as a result of sudden suffocation or acute apnœa. It has commonly been

supposed that the convulsions thus occurring are caused by the noxious influence of black blood upon the brain. It is far more probable that they are caused by the sudden and extreme anæmia of the brain, consequent on the impeded flow of blood through the lungs into the systemic heart and arteries, as explained in my first lecture. The epileptiform convulsions which often result from the inhalation of nitrous oxide gas admit of the same explanation. It is quite certain that, in Kussmaul and Tenner's rabbits, with closed carotids and subclavians, no black blood could reach the brain, yet the convulsions were apparently identical with those which result from suddenly fatal apnœa, whether in the lower animals or in the human subject.

A few years since, the following case came under the observation of my friend Dr. Lavies and myself. A gentleman, about sixty years of age, had been confined to his bed for three weeks with symptoms which pointed to great feebleness of the heart, and probably to fatty degeneration of its walls. There was dyspnœa on exertion, and sometimes on awaking after a long sleep; the heart's impulse and the radial pulse were feeble; there was some œdema of the legs, and over the bases of the lungs there were moist crepitating sounds, probably the result of œdema there. He awoke in the middle of one night, told the nurse that he felt quite comfortable, asked the time, and began to repeat her reply "Oh, half-past ——," when he suddenly stopped, and the nurse, turning to him immediately, saw that his face was livid and he was in strong convulsions. In a few seconds, and before any one could answer her call for assistance, the patient was dead. The body was examined, in the presence of Dr. Lavies and myself, by my friend and former colleague Dr. Kelly. The walls of the heart were thin, soft, and fat. The right ventricle was dilated, and contained firm decolorised thrombus, extending from the apex of the ventricle through the tricuspid orifice into the auricle, to the outer wall of which it had evidently been attached and moulded, but, becoming separated from the auricular wall, it had fallen over the tricuspid orifice and completely closed it. Thus, the circulation must have been completely and instantaneously arrested. The result was lividity of the face from venous fulness, and epileptiform convulsions from cerebral arterial anæmia. In this case, as in the case of the rabbits with ligatured arteries, it is evident that the convulsions were caused, not by black blood, but simply by the absence of circulating blood in the cerebral vessels.

When animals are killed by air being forcibly blown into a vein, the breathing becomes hurried, the animal suddenly falls down, and usually dies in convulsions; the contents of the

bladder and rectum being frequently expelled at the time of death. Dr. John Reid states that, "in very few cases only is death from this cause not preceded by convulsions." (Physiological, Anatomical, and Pathological Researches.)

The immediate cause of death in these cases is the arrest of the frothy mixture of air and blood by the contraction of the pulmonary arterioles, the air seldom reaching the left side of the heart; and as a result of this arrest there is, of course, sudden extreme anæmia of the brain, and of every other organ supplied by the systemic arteries. In man, it appears that death from the accidental admission of atmospheric air into a vein during an operation, is less frequently preceded by convulsions. Probably the chief reason of the less frequent occurrence of convulsions from this cause in the human subject is, that the amount of air accidentally admitted is less, and death consequently is less rapid than when air is forcibly blown into the vein of an animal. It would probably be found, on a careful inquiry, that the occurrence of convulsions in these cases depends upon the circulation being suddenly and completely arrested.

It has been noted, in some cases of suddenly fatal pulmonary embolism, that death has been preceded by convulsions; and Virchow observed, amongst the results of artificial embolism of the pulmonary artery in animals, convulsions and dilatation of the pupil. (Des Emboles Pulmonaires, par B. Ball, page 129.)

We find, then, a large amount of evidence pointing to the conclusion that sudden and extreme anæmia of the brain will cause epileptiform convulsions, and a theory of epilepsy has been framed in accordance with these facts; the theory being that the cerebral anæmia, which is the immediate cause of the convulsion, is the result of spasm of the cerebral arterioles. It may be said with truth that this is only one step towards an explanation of the phenomena, and that the cause of the arterial spasm remains to be determined. We will presently revert to this question.

It is, I think, pretty generally admitted that this theory of cerebral anæmia from arterial spasm is quite consistent with the phenomena of epilepsy. It is a matter of general observation that, at the very commencement of an epileptic fit, the face is pallid. There is obviously anæmia of the superficial vessels, and with this there is probably associated anæmia of the intracranial vessels which supply the brain itself. The pallor is in most cases soon succeeded by lividity, owing to the venous engorgement which results from impeded respiration and pulmonary circulation. It is very remarkable that, while the face is pallid, the heart is beating strongly and the carotids throbbing violently. These phenomena would be explained by

extreme contraction of the muscular arterioles, resisting the escape of blood from the arterial trunks into the capillaries.

Kussmaul and Tenner endeavoured to support the theory of arterial spasm by experiment, and to some extent they succeeded. In each of two white rabbits, they ligatured the two subclavians and one carotid; the cervical sympathetic, on the other side, was then exposed and galvanised, with a view to excite contraction of the arterioles by the stimulus conveyed through the vaso-motor nerves. In two animals, no effect was produced; but in the third, the background of the eye became completely pale; the pupil dilated, so that the iris could scarcely be seen; the neck was drawn back, and violent convulsions occurred. The electrodes being removed, the spasms ceased, the pupil contracted, and the background of the eye became red; but the animal continued in a swooning condition. After some minutes, electricity applied to the sympathetic nerve produced the same effect as at first. A third attempt to excite convulsion did not succeed.

The authors suggest that these experiments deserve repetition, with a view of rendering certain what at present is probable, namely, "that epileptic convulsions can be brought about by contraction of the blood-vessels induced by the vaso-motor nerves."

According to this theory, then, epilepsy is the result of sudden anæmia of the brain; and this anæmia, when not caused by a sudden and profuse hemorrhage, or by some impediment to the circulation outside the cranium, is due to an extreme contraction of the muscular arterioles. This arterial contraction may be determined by two main classes of causes:

1. By a purely nervous reflex influence, such as, for example, may be excited by anger or terror, by the irritation of the gums during dentition, by a calculus in the kidney, the ureter, or the gall duct, or by worms in the intestines.

2. In the second class of cases, a blood-poison is the exciting cause of the arterial spasm and the resulting epileptic convulsion. This includes all cases in which convulsions result from retained excreta, of which uræmic convulsions are a typical example.

From the preceding narrative of facts, it appears to be highly probable that uræmic convulsions are directly due to a sudden and extreme anæmia of the brain, resulting from contraction of the cerebral arterioles, and that the arterial contraction is excited by the influence of impure blood upon the vaso-motor nerves and centre.

This theory, moreover, indicates two modes in which uræmic convulsions may be prevented, namely: first, by means directed towards removing the morbid quality of the blood; and,

second, by remedies which lessen the reflex excitability of the nervous centre.

Time would not permit, even if it were desirable to enter into the details of treatment; but I am anxious to direct attention to one or two points of practice. It is a well-known fact that the inhalation of chloroform or ether-vapour invariably puts a stop to uræmic convulsions, and often wards off an attack after premonitory symptoms, such as convulsive twitchings of certain muscles, have occurred. It has sometimes been supposed that the anæsthetic acts by relaxing the cerebral arteries; but an observation of Kussmaul and Tenner points to a different explanation. These experimenters found that, if animals are etherised, no convulsions occur when they are bled to death, or when their intracranial circulation is arrested by ligatures. It appears, therefore, that the anæsthetic vapours prevent or stop convulsions by lessening the reflex excitability of the nervous centre.

The undoubted influence of repeated full doses of bromide of potassium, in warding off uræmic convulsions, is also probably to be explained by its soothing sedative influence on the nervous centres. The bromide is a very useful remedy for the painful muscular cramps which are of common occurrence in the advanced stages of all forms of renal degeneration. These cramps, which are especially frequent and severe in the lower extremities and during the night, are no doubt to be classed with the results of uræmic poisoning, and in not a few cases they are the precursors of more formidable nervous disorder. They may, in some cases, be entirely prevented by a draught containing twenty grains of bromide of potassium, with five grains of carbonate of ammonia, at bed-time.—*British Medical Journal, May* 12, 1877, *p.* 577.

DISEASES OF THE ORGANS OF CIRCULATION.

16.—ON THE POSITION AND MECHANISM OF THE HÆMIC MURMUR.

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Inorganic Cardiac Murmurs—that is, murmurs audible over any part of the cardiac area, and occurring independently of any irremediable mechanical alteration of the organ—have been classified by authors under two heads: hæmic and dynamic. Hæmic murmurs have been regarded by all authorities as systolic in their rhythm; and by most, as basic in position; and as produced by the passage of an abnormal

blood through a perfectly normal heart, after a fashion which has been variously explained. Dynamic murmurs, on the other hand, though also systolic in their rhythm, yet have their position at the apex, are regurgitant in character, and are supposed to be produced by a perturbed or defective action of the cardiac muscle itself, independent of any abnormality of the blood, which, however, probably always co-exists.

Chlorosis, and the chlorotic cardiac murmur, is the typical example of the one class; chorea, and the apex murmur that so often accompanies it, the typical example of the other class. In the one set of cases the blood lesion is primary and paramount; in the other set the blood lesion is essentially secondary, though not always actually so, and it forms but one link in the chain of consecutive causes of the morbid phenomena. The essential lesion of the blood in both of these classes is deficiency in its nutritive qualities; and, in the latter class, we have superadded the exhaustion consequent upon almost incessant muscular action, or upon co-existent or pre-existent febrile disorder. This state of matters, as we all know, produces an enfeebled, relaxed, and flabby condition of the muscles, which, of course, is not confined to the external muscles, but extends also to the hollow internal muscles, which suffer proportionately to the work they have to do. In these conditions the heart necessarily participates, and we are not therefore surprised to learn that in fevers it sometimes becomes as limp as a piece of wet paper (Stokes), nor that in serious cases of chlorosis it is found to be considerably dilated (Bamberger, Friedreich, Wunderlich, Stark, &c.) Beau, in 1856, associated fevers of various kinds with chlorosis and other forms of hydræmia as causes of dilatation of the heart, pointing out that it was rational to attribute this dilatation to that peculiar alteration of the blood which is common to all these different forms of disease. Naturally he was the more inclined to adopt this view, as, so long ago as 1845, he had experimentally proved that death following the hydræmic condition of blood induced by repeated venesections, in dogs and rabbits, was accompanied by a dilated condition of the heart. Dilatation of the heart, however, is, as was first pointed out by Stokes, and subsequently by Gairdner and Bristowe, a very frequent cause of systolic apex murmur. Stark has shown that the dilatation of chlorosis may be accompanied by relative inadequacy of both the mitral and tricuspid valves, and that murmurs of regurgitation may thus be heard at either of the auriculo-ventricular orifices; while Parrot has given it as his opinion that the true hæmic murmur is invariably a murmur of tricuspid regurgitation. The chlorotic or typical hæmic murmur is thus shown to be invariably accompanied by a con-

dition of heart which may give rise to a dynamic apex murmur; while, in the opinion of some, the so-called hæmic murmur is always of this character. It is true that the choreic, or true typical dynamic, murmur has been alleged to be caused by "disordered action of the muscular apparatus connected with the valve" (Walshe.) But while clonic spasm of one or other of the musculi papillares might give rise to an intermittent murmur, there is some difficulty in recognising such spasm as an efficient cause in producing a mitral murmur of such uniformity as the ordinary curable choreic mitral murmur possesses. Besides, "there is no good proof that involuntary muscular organs participate in the muscular disorder" (Kirkes); while "the admitted co-existence of rhythmical action of the substance of the heart with this alleged tetanic and irregular contraction of the papillary muscles, which, as proved by dissection, are directly continuous with the fibres of the ventricular walls, constitutes a still stronger objection to this theory" (Hayden), especially if we "bear in mind the unity of nerve-centres, and the community of nerve-distribution enjoyed by both portions of the same fibres." Moreover, the extension of this explanation to the dynamic murmur of weak and fatty hearts, in which there is not the slightest evidence of nervous disturbance—as has been done by Walshe—is quite inadmissible; while the explanation of "parietal debility of the left ventricle," adopted by Hayden, seems to be the only one applicable to both classes of cases, is perfectly sufficient, and is consonant with all the facts of these cases. But "parietal debility" is merely another expression for that muscular relaxation resulting in dilatation, which, as we have seen, is so universally recognised as a tolerably constant concomitant of various forms of fevers as well as of chlorosis. Thus we have the dynamic apex murmur of chorea associated in its cause with that occurring in various forms of fever, as well as in certain cases of chlorosis. Chlorosis, in its severer forms, is therefore, and has long been, acknowledged to give rise to a dynamic apex murmur, of a perfectly similar character with that so often occurring in the course of chorea, the eruptive and other fevers, and originating in a precisely similar manner—namely, in relative inadequacy of the auriculo-ventricular valves, the result of dilatation of the ventricles consequent on parietal debility. It is of consequence to remember that this dilatation is not active, in which case it would rather be a sign of increased than of diminished ventricular power; but that it is passive, the gradual result of residual accumulation in the cavity of a hollow muscle too feeble fully to discharge its ordinary function. It is also important to remember that dynamic regurgitation in its results differs in no respect from regurgita-

tion due to organic disease, but, equally with the latter, is always followed by pulmonary congestion, as well as by all the usual consecutive results of obstruction to the circulation originating in the left heart, all of which may be detected with greater or less ease in proportion to the amount of regurgitation present, and the time it has persisted.

Bearing all these facts in mind, we are tempted to inquire how it is that the hæmic murmur of chlorosis becomes, in its later stages, an apex murmur of regurgitation, after having been in its earlier stage a basic murmur — apparently of obstruction. To this query there is, I think, but one reply, and that is that, all through, the murmur is due to ventricular dilatation, and the changes in its position of maximum intensity are due to alterations in the heart itself. I am quite aware that alterations as to the position and textural condition of the media overlying and surrounding the heart, bring about many remarkable changes, both in regard to the production and the propagation of murmurs. Into these, however, it is at present unnecessary to enter, except in regard to one or two points subsequently to be referred to.

In chlorosis, the prime evil, so far as the heart is concerned, is unquestionably the spanæmic condition of the blood, however that may have been brought about. In this state the great defect is the diminution of the cellular element, the serum remaining more or less normal; sometimes it is increased in amount, but it is never diminished, though occasionally its constituents are more or less altered. Thus, even when at rest, the heart has at least as much work to do as usual, and sometimes it has more, while it is much less able to do it; and whenever any exertion is made the breathlessness due to the state of the blood necessarily calls upon the heart for more than usual effort, which it is less than usually able to exert. The result of this is easily foreseen; residual accumulation commences, and gradually increases, in the left ventricle, for it is there that the strain primarily falls, and it is the left ventricle which suffers first and most in these and similar cases, as we learn from Stokes and others. By and bye the dilatation increases so much that a relative inadequacy of the mitral valve is established, and regurgitation is set up. We must not forget, however, that during all this time a similar process of congestion, from residual accumulation, has been gradually going on in the left auricle first of all, next in the lungs, and then in the right ventricle, terminating, of course, in the systemic venous system, where all such processes naturally end. This process is essentially slow and gradual, but it is constant, and proceeds *pari passu* with the changes in the left heart, the passage to the

right heart being probably not long delayed by the distensibility of the pulmonary vessels. It is important also to remember that, in accordance with a well-known law, some degree of hypertrophy is set up in probably every case, but this must necessarily be imperfect, and must lag much behind the tendency to dilatation.

From these preliminary explanations it is obvious that in chlorosis we may have a murmur at all the four cardiac orifices; but I am quite certain that most observers will agree with Hayden that its most ordinary position is the base of the heart, and that it has no "definite line of propagation, as is the case with organic murmurs in this situation." This absence of any distinct line of propagation up the aorta, innominate, and carotid, of course at once renders untenable the classic hypothesis of Hope, Bellingham, Potain, &c., who hold that the cardiac hæmic murmur is always aortic in its origin. Marshall Hughes, feeling the force of this objection, was, I believe, the first to suggest the orifice of the pulmonary artery as the source of the murmur, forgetting that under ordinary conditions any purely hæmic, intracardiac cause of murmur must act at least as forcibly at the aortic orifice as at the pulmonary, And Marey did not improve matters by suggesting that the basic murmur was due to the formation of fluid veins at the aortic orifice, by the lowering of the tension within the aorta itself, because most observers are agreed that the murmur is at least as loud at the pulmonary orifice as at the aortic, while the accentuated pulmonary second indicates an increase of tension within that artery—a condition also proved to exist by the gradual development of dilatation of the right ventricle. The cause, therefore, supposed to favour the production of the murmur at the aortic orifice is wholly inoperative at the pulmonary orifice, where nevertheless the murmur earliest makes its appearance and is usually loudest.

This basic position of the chlorotic murmur in its early stage of course excludes Parrot's supposition of its being always tricuspid in its origin; nevertheless, I acknowledge that a greater or less dilatation of the right ventricle, as well as a more or less evident undulation or pulsation of the jugular veins, are always present, though I think that most observers will agree with me that a systolic tricuspid murmur is the rarest of all the murmurs which may be found in connexion with chlorosis, unless, indeed, we regard the murmur not unfrequently found in the mitral area in advanced cases as of tricuspid origin, which there are some good reasons for doing.

We must not also forget that although the reasons already stated preclude the idea of the origin of the chlorotic murmur in its commonest form at either the aortic or the pulmonary orifice

from ordinary causes, yet it might possibly arise at the pulmonary orifice from compression, during the ventricular systole, of the artery uncovered by retraction of the lungs, as has been pointed out by Quincke. In all cases. however, where the murmur depends solely upon this cause, and the lungs, as in chlorosis, are texturally sound, full inflation of them causes the murmur completely to disappear. But this is not the case in chlorosis; a full inspiration with subsequent holding of the breath certainly makes the murmur somewhat fainter, but never causes it to disappear.

Besides, however, all those causes of basic murmur already shown to have no influence in the production of the chlorotic murmur, in its early stage at least, there is one other of more recent discovery which in its essential nature, the position of the murmur to which it gives rise, and its mode of propagation, exactly fulfils all those conditions which we find associated with the chlorotic murmur.

Skoda mentions that in cases of dilatation and hypertrophy of the right ventricle, dependent upon incompetence of the mitral valve, a systolic murmur is often audible in the pulmonary artery; this, he adds, is "perhaps" dependent upon softening (*auflockerung*) of the internal coat of the dilated artery. So far as I know, Skoda was the first to point out the existence of this murmur, which has long since been recognised as of no infrequent occurrence in mitral regurgitation, whether accompanied by stenosis or not. If stenosis be present, it is somewhat remarkable to hear, as we often do in such cases, a presystolic murmur over the apex, and a systolic one in the region of the pulmonary artery. Skoda's idea as to the causation of this murmur was not long maintained; it was not difficult to show that the internal coat of the pulmonary artery in these cases is not softened, and that its condition in those cases where this murmur had been present does not differ from that in those other cases where this murmur had been absent. Meyer, who next took up this question, endeavoured to show that this murmur was produced by the propagation of the vibrations of the mitral valve to the wall of the pulmonary artery. But, supposing this to be an efficient cause, it is difficult to see why these vibrations should not be propagated to the aortic wall which is attached to the same fibrous ring, rather than to the pulmonary artery which is attached to the same fibrous ring with the tricuspid valve, and belongs, as it were, to a different heart. If it were produced in this way, we ought to have the systolic murmur loudest in the aortic area in these cases of mitral regurgitation, instead of in the pulmonary area; but this we know not to be the case. Bamberger supposed this systolic murmur, audible in the pulmonary area in cases of mitral

regurgitation, to be due to an abnormal relaxation of the coats of the pulmonary artery. It may be well to give his own words. He says this murmur "seinen grund höchst wahrscheinlich in der durch die andauernde Erweiterung bedingten Erschlaffung der Gefässhaute hat." And I especially quote Bamberger's words, because Naunyn has said—"Bamberger und ihn folgend Gerhardt geben an; es sei das bei mitralis insufficienz in der gegend der Pulmonal Klappe hörbare systolische Geräusch eine folge der abnorm *hohe Spannung*, in welche die wandung der Pulmonalarterie unter solchen Umständen versetzt würde." I need not say that Bamberger's own view is utterly devoid of any rational foundation whatever; while, on the other hand, it is unnecessary to discuss the opinion ascribed by Naunyn to Gerhardt, as he, as well as Paul Niemeyer, have now both adopted the view originally propounded by Naunyn himself. By a careful examination of these cases, Naunyn appears to have distinctly made out that the position of maximum intensity of the murmur in them is not over the pulmonary artery at all, but one or two inches to the left of it, and in the same plane, just where anatomy teaches us that the appendix of the left auricle comes up from behind. Naunyn therefore regards this murmur as propagated to the chest wall from the left auricular appendix; and he supposes the vibrations which constitute the murmur to be conveyed to the auricular wall by the fluid veins formed by the regurgitating blood at the auriculo-ventricular opening, which carry with them the vibrations originating at that orifice to the auricular wall upon which they impinge. I entirely agree with this explanation given by Naunyn; and though he has not specially referred to the chlorotic murmur as originating in this way, it is not, I think, difficult to show that the history of the development of this murmur, as well as its primary position of maximum intensity, fully confirm this theory of its causation.

It is a difficult matter to explain why a murmur of regurgitation, which always originates at the auriculo-ventricular opening, should be at one time audible at the apex and at another at the base. But some approximation at least to an explanation may be attained if we consider that sounds are only audible vibrations, that in fluids these vibrations originate only at some part where the stream is constricted, and are not only communicated to the constricting walls at the point of origin, but are also carried with the fluid veins and transferred by them to any solid upon which they may impinge. In these circumstances it is obvious that a murmur of mitral regurgitation may be audible either at the apex or the base, according to the relative readiness with which its vibrations are transferred to the chest-wall in either situation. If the vibra-

tions are powerful, they are readily propagated to the ventricle, and make their appearance as a distinct murmur at the left apex, which under most circumstances of mitral regurgitation comes more directly in contact with the chest-wall than the auricle—at first, at all events,—and for long continues to do so, because the cardiac muscle in these cases being fairly firm, consecutive dilatation is only very gradually developed. It is quite otherwise in chlorosis. In this disease residual dilatation is the essential lesion of the heart, and the murmur only becomes developed as a late result. By this time the left auricle, which very early commences to dilate, has become so enlarged that the appendix is frequently visible as a pulsating tumour between the second and third ribs on the left side, while the left apex is gone, separated from the chest-wall by the dilated right ventricle lying in front. How trifling a dilatation of the right ventricle is sufficient to produce this displacement of the left apex, even in the normal state of the heart, may be readily ascertained by anyone who places his hand over the ventricular area and holds his breath. In a very few seconds the left apex will be found completely to disappear, and that without any perceptible development of the impulse of the right ventricle. So it is in chlorosis; the right ventricle speedily becomes sufficiently dilated to displace the apex beat without its own impulse being developed in any marked manner, though that ultimately follows in due course. By this time, or soon after, the left ventricle is so dilated that during its systole the edges of the mitral valve are unable to meet, and regurgitation results. The fluid veins thus formed are of low tension, the vibrations originating at the auriculo-ventricular orifice have little force, so that they are propagated but feebly to the wall of the left ventricle, which in its turn, from its deep-lying position, transmits them with difficulty to the chest-wall, where they become audible only as an impure first sound. On the other hand, these vibrations readily pass with the fluid veins to the wall of the left auricle on which they impinge, and the auricular wall being already somewhat tense from unusual dilatation, and being also, from the same cause, in more than usually close apposition to the chest-wall, these vibrations are readily transmitted to it, and become audible as a soft murmur in the area of the auriclar appendix, around which it is propagated to an extent commensurate with its own loudness and the resonance of the chest-wall.

Various other circumstances may combine to facilitate this mode of propagation of the murmur of mitral regurgitation, but the conditions described are certainly efficient, and are more or less present in all cases in which this mode of propagation is observed.

By and by, however, without there having been any disappearance or diminution of the basic murmur, the impure first sound at the apex becomes changed into a loud, blowing murmur. And this may occur in two ways: first, the dilatation and subsequent hypertrophy of the left ventricle may be so increased that the vibrations originating at the auriculo-ventricular orifice become forcible enough to be transmitted to the chest-wall in the region of the apex; or, second, the right side may become so dilated that its apex occupies the normal site of the left apex, and a tricuspid may thus be mistaken for a mitral murmur. Both kinds of cases may be met with, and require to be differentiated one from the other.

There is no doubt, also, that cases occasionally do occur in which we have—besides the auricular and any other murmur which may be present—a distinct systolic murmur originating in the aorta and propagated along the arteries; and there is every reason to believe that the same thing may occur in the pulmonary artery, quite apart from any external source of pressure in either case, and from internal causes alone. This will be found to occur at a somewhat advanced period of the disease, when the primary dilatation of the ventricles has been followed by some degree of hypertrophy; so that, as Beau has pointed out, a larger wave than usual is thrown into the arteries, not only therefore producing vibration waves in the arterial coats themselves, but very possibly also forming fluid veins at the arterial orifices, from the relative constriction produced there by the abnormal friction of the chlorotic blood—a relative constriction which, in some cases at all events, may be supplemented by some slight dilatation of the aorta itself, due to relaxation of its tissues. For the production of the aortic systolic murmur, therefore, in such cases, we have the possible occurrence of two efficient causes, sometimes one of them, and at other times the other, predominating; while of course similar causes must coexist in similar relation to the pulmonary orifice, and be followed there by similar results.

The sequence of the cardiac phenomena in chlorosis is entirely in accordance with the pathological view just propounded. Whatever may be the ultimate cause of the chlorosis, *spanæmia* is its first evident symptom, and of this the earliest sign is a *venous hum—bruit de diable*—in the right jugular vein. As the result of this spanæmia, the cardiac muscle becomes relaxed, and its cavities consecutively dilated from residual accumulation. The earliest indication of the delay of the blood, resulting from this residual dilatation, is *accentuation of the pulmonary second*. This is speedily followed by a *systolic murmur* in what I have termed the *auricular area*, an inch or two to the left of the pulmonary area, and in the same plane where a distinct

pulsation is usually both to be felt and seen. This murmur radiates of course all round its position of maximum intensity, but is most readily propagated to the right from the resonant property of the sternum. With this murmur there is always associated *an impure first sound in the mitral area*, and in many cases this gradually passes into a *systolic murmur*. When the chlorosis is cured these murmurs disappear, precisely in the reverse manner to that in which I have described them as arising, every trace of the auricular pulsation vanishing long before the venous hum ceases to be heard. This systolic auricular murmur coincides in time with the systole of the ventricles, its rhythm is ventriculo-systolic, its position basic, differing in both, *toto cœlo*, from the so-called presystolic murmur, which is apical in position, and auriculo-systolic in rhythm, coinciding with the time of the contraction of the auricles; the former murmur is also soft in character and high in pitch, the latter rough and low. In chlorosis this auricular murmur is always present, though it may be associated with other murmurs, which, as I have shown, may arise in this condition at all the four cardiac orifices. Whenever we have spanæmia we have a tendency to residual dilatation, and consequently to the development of this murmur, which therefore may be heard in chlorosis, chorea, all the exanthematic and other fevers, in syphilis and all other diseases known to be accompanied with spanæmia, as well as in the exhaustion following excess in tobacco-smoking or sexual indulgence. Where the residual dilatation is the result of extraordinary or unusual exertion—heart strain as it is termed, which may be the result of arterial atherosis alone, the extra exertion being confined to the cardiac muscle—this murmur is often the only detectable sign of the commencement of what may, if neglected, terminate in serious cardiac disease; the only other indications of an affection which may prove so serious being uneasy feelings in the left breast, which are too often pooh-poohed as nervous, air and exercise being recommended for a heart already unable to perform its ordinary lifework. As dilatation is the most serious affection that can befall the heart, and as this auricular murmur is the earliest detectable indication of dilatation, I need not say that I regard it as perhaps the most important sign we possess of cardiac disease, all the more so that it is an early one, while I look upon its detection as a most valuable indication for treatment, a treatment which of course must be varied according to the other concomitant phenomena.

The auricular pulsation between the second and third ribs to the left of the sternum, which is so prominent a phenomenon in many cases of chlorosis, is sometimes so well marked that it is mistaken for serious arterial disease. A case of this kind was

sent to me in 1869 by the late Dr. Hughes Bennett as one of aneurism of the aorta. This case was one of the first which gave an impetus to my inquiries as to the causes of the murmurs and of the pulsations in the pulmonary area. Treated at first as a case of aneurism, this patient was subsequently recognised to be a case of simple chlorosis with auricular pulsation. She was discharged cured. I have seen her frequently since, and I saw her a few months ago perfectly well and free from all trace of murmur or pulsation.—*Lancet, Sept.* 18, 1877, *p.* 383.

DISEASES OF THE ORGANS OF RESPIRATION.

17.—ON TUBERCULOSIS.

By Dr. Arthur Wynne Foot, Senior Physician to the Meath Hospital and County of Dublin Infirmary.

By the term tuberculosis—first employed by Schönlein in the third decade of the present century—is meant a certain morbid state of the liquids of the organisation, which often spreads through the body from a single point, but which may perhaps in some cases be congenital. This morbid state finds its anatomical expression in the growth called tubercle. This tubercle or tubercular granulation, under some one or other of its forms, constitutes the characteristic and fundamental lesion in acute or chronic phthisis; and hence has arisen the necessity of once more enlarging the domain of pulmonary tuberculosis from the somewhat narrow limits within which, subsequent to the time of Laennec, it has come to be circumscribed.

There are not, as yet, any anatomical, histological, or chemical criteria sufficiently constant to base thereon singly a definition of tubercle which shall be adequate and complete, and in our present state of knowledge it is premature to make the attempt. Neither to the scalpel, the microscope, or the laboratory has the tubercular granulation as yet revealed any such uniqueness of structure or property as will serve to stamp it with an individuality and discriminate it from every other new formation. The consequences of this inability to frame an abstract definition of tubercle are not so disastrous as the captious questioner of what *is* a tubercle may think. The anatomical and vital characters of tubercle, taken collectively, and intelligently judged of, are, although not specific, distinct enough to characterise the formation.

There are certain difficulties in the practical anatomy of phthisical lungs which can be remedied only by some acquaintance with the vital history of the tubercular granulation. Thus it is necessary to bear in mind that the grey, semi-transparent granulation, the most obviously characteristic phase of tubercle, is only an early stage of the growth; otherwise one who is

wont to form his opinion that a lung is tuberculous solely from the presence therein of the grey granulation, may be looking at a tuberculous lung and yet see no tubercle, because he has arbitrarily selected a particular phase of the growth as his criterion of its presence. It is true that the grey granulation, although by no means the sole condition in which tubercle may occur, embodies its most distinctive character to the naked eye; it is also under such a form that tubercle is most commonly met with in the cases of acute disseminated tuberculosis, about the nature of which there is little difference of opinion; and, furthermore, this tendency to withhold the name of tuberculosis, unless the grey granulation can be seen with the naked eye, is greatly due to the influence of Rokitansky's earlier teaching, in which he declared emphatically his dissent from the opinion that grey tubercle, the grey tuberculous granulation of Laennec, softens. It is now well established that the grey granulation is quickly destined to undergo changes in its colour, consistence, and composition. In its early stages miliary tubercle is identical in all the organs in which it is met with, but its subsequent appearances essentially depend upon its development, period of maturity, and complicity with secondary results. The grey granulation, in consequence of the extreme perishability, and of the very limited viability of its living elements, as a rule, regularly undergoes the caseous metamorphosis and subsequent softening, so that it is necessary to bear in mind this frequent change, and to recollect that young tubercle comports itself differently from older tubercle, that the former is viable and living, the latter often only detritus and disorganisation. This necrosis and fatty change in the older tubercle is, as was just now observed, connected with the eminently feeble vitality of its corpuscular elements, tubercle being, in Virchow's words, always a pitiful production, a new formation from its very outset miserable; it is, however, also partly due to insufficient nourishment of the growth through the vessels round it. It is well known that it is impossible to inject a real tubercle, and that there are never blood-vessels containing blood within one, which is one of the reasons for the pale, semi-transparent appearance of the fresh granulation. The capillaries of a tissue in which the tubercular nodule is being formed become gradually obliterated in the process of its growth, and there is no new formation of vessels such as takes place in more highly developed inflammatory tissue. The obliteration or destruction of the blood-vessels in tissues which are the seat of tubercle depends upon the enormous new-formation of nuclei around the capillaries, as well as upon the increase of nuclei which takes place in the walls of the vessels themselves. At what age the tubercle begins to

undergo its retrogressive changes has not been determined; for the most part, a few weeks appear to suffice to call it forth, at least in the centre of the tubercle, while at other times months are necessary. Besides the more usual caseous change, the tubercular granulation is also subject to a metamorphosis into fibrous tissue. Fibrous tubercle is, according to some, a peculiar form of tubercle, according to others only a transformation of it. Virchow distinguishes fibrous from common cellular tubercles; according to Langhans the former, or fibrous tubercle, is a more highly developed form of cellular tubercle, while, according to Schüppel, fibrous is a metamorphosis of common tubercle. There is, moreover, that condition of miliary tubercle called cornification, which is effected by simple atrophy of the nuclei alone (without simultaneous fatty metamorphosis), whereby the tubercle is transformed into a hard, horny mass. The physical variations, then, to which the grey granulations are subject cause a difficulty of recognising evidences of their presence in chronic tuberculosis to those who know of them only in one stage, and that not the most frequently met with, of their existence. They cause, also, the more excusable difficulty of distinguishing tubercle in its latter end from products of different origin.

Again, in examining the lungs of tuberculous patients, the fact is sometimes overlooked that the smallest forms of tubercle are not visible to the naked eye. The term miliary is a conventional one, conveying a definite idea of size, that of a millet seed, to which Mangetus, as early as 1700 compared the tubercles he found in the lungs and other organs; but there are tubercles as genuine, much smaller. Virchow has already demonstrated that tubercles as large as a millet seed are infrequent, while those of the size of a *poppy* seed are much more common. He has shown that the smallest nodules visible with the naked eye are made up of yet smaller nodules, the submiliary tubercles. These measure from one-fifth to one-seventh of a millimetre in diameter, so that from forty to fifty of them must be conglomerated to make a nodule as large as a poppy seed. While there are no limits to the size which may be attained by the agglomeration of submiliary tubercles—witness the solitary tubercle of the brain, which may occupy one half of the cerebellum — and while tubercle can exist invisible to the naked eye, it is evidently highly unsafe to assume any particular *size* of tubercle as a criterion, or to hastily assume, without a careful microscopical examination, that the lung is free from such a growth, because there are no miliary granulations apparent, no tubercle *visible* to the naked eye.

It is almost needless to remind those who are at all familiar

with the manipulation of tuberculous lungs of the intrinsic difficulties of examining these organs, independently of the arduous task of detecting tubercle which may be invisible to the naked eye, disguised under some of its metamorphic conditions, or concealed among the secondary results of its presence. The anatomical peculiarities of the lungs are such as eminently favour the more or less simultaneous occurrence of different kinds of lesions under the agency of inflammatory or irritative influences. Thus, in examining tuberculous lungs, besides the initial and characteristic lesions—the tubercular granulations, we must be prepared to meet, however short a time the disease has lasted, with inflammations common and special of the bronchi, the air-vessels, the pleuræ, and of the lymphatic glands. These different lesions, consecutive to the tubercular granulation, may even become predominant in an anatomical and clinical point of view. This series of changes in tissues surrounding tubercles is dependent upon the quick formation of the tubercle, upon the obliteration of the capillaries, upon the continuance of the small-celled growth on the sheath of surrounding blood-vessels, upon the frequently occurring collateral hyperæmia and hypertrophy of the surrounding tissues, and upon the retrograde metamorphosis of the tubercle itself. In short, excluding cancer, the lung in phthisis may offer a compendium of all the pathological accidents to which that organ is liable; and differences thus arise enough to confuse those who seek for variety, while meantime in all cases the initial and leading features of the disease are identical.

In the year 1850, twenty-four years after the death of Laennec—for Laennec died of phthisis 13th August, 1826, in the forty-fifth year of his age—appeared Reinhardt's work, which was the result of the application of the microscope to these tubercular infiltrations. The great merit of this work was the establishment of the fact that the major part of the consolidations known as grey and yellow infiltrations were due to intra-alveolar and interstitial lesions, the result of pneumonia, and so far he brought histological evidence to support the opinion of Broussais that tuberculosis was only a particular form of pulmonary inflammation—the very view which Laennec had so rigorously combated. But Reinhardt's conclusions were not free from the errors which ever dog the slow advance of scientific progress. He denied the transformation of the grey into the yellow infiltration, and seems to have overlooked the existence of the grey granulation or typical tubercle. Virchow soon followed, correcting Reinhardt's mistakes, maintaining the importance of the pneumonic process, rigidly restricting the tubercle to the typical grey miliary granulation, and altering the nomenclature of Laennec. What had hitherto been called

tuberculous matter was now called caseous matter ; and tuberculisation, or that condition which brings about tubercular infiltration, was called caseation, and the peculiar pneumonia now known to accompany the phthisical processes was called caseous or scrofulous pneumonia. The tubercular element was completely subordinated to the inflammatory processes. Virchow had taught that the caseous metamorphosis, the hitherto-called tuberculisation, which Laennec regarded as a special attribute of tubercle, was to be looked on as a general process of necrobiosis in all tissues and exudations. Virchow's teaching that all cheesy matter is not tubercular has, from the preponderating influence of such a master, been since pushed to so dangerous an extreme, that many are even disposed to doubt the tubercular nature of any caseous matter, whereas, on the contrary, tubercle is one of the most frequent although not the sole cause of such productions in the body.

In the diffused consolidations of the lungs, called tubercular infiltration by Laennec, much of the caseous matter is unquestionably pneumonic, the result of pneumonia running a particular course in the presence of tubercle. It is the presence of the tubercular growth which gives to phthisical pneumonia that characteristic tendency to caseation which does not occur in ordinary acute inflammation of the lungs, or in broncho-pneumonia uncomplicated with tubercle. This combination of inflammation and tuberculosis, which comports itself differently from ordinary pneumonia on the one hand and from mere tuberculosis on the other, is characterised pathologically as well by the tendency to caseation exhibited by the combination of the pneumonic products and the tubercle, as by the tubercular development which can be discovered thereabouts when properly looked for. The combination of inflammation with tuberculosis is recognised in other organs, and is implied by the names in common use, such as tubercular meningitis, tubercular arthritis, pleuritis, epididymitis. Inflammation in such cases is characterised by new formation of connective tissue, which is met with either in the form of granulation-tissue rich in cells, or which is poor in cells, and of older date. There lie imbedded therein tubercles of submiliary or microscopic size, which may also by agglomeration reach the size of a pea and larger ; in the former case they are fresh and grey, in the latter older and yellow. The abundant nuclear and cell-formation arising under the influence of inflammation in persons affected with tuberculosis very soon suffers cheesy metamorphosis, and assumes a molecular state, whereby destructions of membranes or parenchymata ensue ; in the former case leading to the tubercular ulcerations, in the latter to tubercular cavities. In a large proportion of cases in which extensive tracts of the

lung are found completely caseous, the condition of the parts is due to tubercular pneumonia—that is to say, to pneumonia complicated with a growth analogous to that existing in the granulations found in acute tuberculosis. Anatomically pneumonia is not tubercular unless this growth co-exists in the alveolar walls. Whenever this growth is traced in the walls of the air-vesicles the capillary circulation is found to cease, and even in a non-injected specimen it can be seen that there are no capillaries there, and that the tubercular growth is occupying their places, whereas in pneumonia uncomplicated with tubercle the capillaries are present. The implication of the alveolar walls to an extent, or for a length of time insufficient to produce the death of the tissues concerned, yet gives them a resistance and at the same time an exsanguine appearance which constitutes the grey infiltration of Laennec. The yellow infiltration is the same thing plus caseation, the anatomical sign of chronicity.

The tendency of modern opinion is to regard the infiltrated tubercle of Laennec as principally indebted for its characters to causes of an inflammatory nature, to consider it as the result of tubercular pneumonia—pneumonia complicated—and leaving the cheesy stamp in consequence of that complication with tubercle.

The lymphoid or adenoid nature of tubercle may perhaps throw some light upon a fact which many have tried to explain, hitherto without much success—namely, the great frequency of tubercular formations in the lungs. The word lymphoid, introduced by Virchow, expresses a histological character of tubercle in which its structure resembles, but is not identical with, an isolated lymph follicle. It has been contended that the term is not strictly applicable, because the essential part of a lymph follicle, according to His and Frey, is its reticulum, which tubercle may or may not exhibit, and generally does so only when in a very early stage; moreover, Ranvier and others say that the reticulum in tubercle, when present, is altogether due to the preliminary hardening which the tubercle has been subjected to for microscopic examination. It has also been objected that a lymphatic follicle is vascular, whilst a tubercular growth is absolutely, or nearly absolutely, non-vascular. However, there is a very striking similarity between tubercles and lymphatic follicles in the size and shape of their cellular constituents, and there is reason to believe that the pulmonary organs are peculiarly rich in lymphatic tissue. The researches of Sanderson have shown that this lymphoid, adenoid, or lymphatic gland tissue is largely met with as a natural occurrence in the neighbourhood of the minute bronchioles. There are lymphatic rootlets in the alveolar septa which anastomose with

the true perivascular lymphatic vessels which are situated in the adventitia of the ultimate branches of the pulmonary artery and vein, generally a couple of them to each blood-vessel, and which finally enter the root of the lung together with the veins. The net-work of peribronchial lymphatics, formed in the adventitia of the bronchi, are also in communication with the perivascular lymphatics, and the efferent vessels of both are identical. Buhl has also shown that the alveolar walls are intimately connected with the lymphatic capillaries, and that the large cells lining these walls correspond with the lymphatic endothelium. Rindfleisch had his attention aroused, in the case of miliary tubercles peculiarly abundant in and upon the interlobular septa, to the singular coincidence between their arrangement and the course of the lymphatic vessels; a step further brought him to the conclusion that the canals from whose walls the miliary nodules grew were no other than the lymphatics, and he describes tubercle of such an origin under the name of lymphangîtis nodosa, and figures it under that of lymphangitis tuberculosa. Sikorski has announced the discovery of a lymphatic plexus in the air-vesicles which presents the closest analogy to the origin of lymphatics in other parts, and which he has traced also through the bronchioles.

There is no contradiction implied in the derivation of tubercle from lymphatic irritation to the opinion of Virchow that tubercle has its origin in connective tissue, as it has been shown by Sertoli and Schmidt that the lymphatic structures, in their embryological development, proceed from the connective tissue series. The most recent works also of His, Schweiger-Seidel, Ranvier, and Rouget, represent the lymphatic system as originating in the connective tissue. Now as to the predisposition of the lungs to tuberculosis, Niemeyer considers that the fact that the lungs more frequently than any other organ become the seat of tubercles, and that this tuberculosis remains very often limited to the lungs, has its cause in this, that the diseases of the lungs, much more easily than those of other organs, leave cheesy products behind them, and that in most, although by no means in all cases, the eruption of tubercles remains confined to certain not very wide limits. This view is a special outcome of the doctrine of pneumonic phthisis. Graves suggested that, although there was no satisfactory solution of the problem why tubercles should be so much more common and numerous in the lung than in any other organ, that it might throw some light upon this obscure subject to remember that the lung is the only organ through which the entire mass of the blood circulates. "Tubercles," he observes, "are a disease of nutrition, a process which depends intimately on the blood; and it may not seem strange," he continues, "that they should

be most frequent and numerous in an organ which has a more intimate connexion with the sanguineous circulation than any other." Tubercle, according to Wilson Fox is an overgrowth or hyperplasia of lymphatic tissue, resulting from irritation of the lymphatic elements excited by abnormal local or constitutional conditions, or in the great majority of cases by both combined. The explanation then may be expressed in a series of propositions. The lungs are peculiarly rich in lymphatic tissue, and are subject to the circulation of the whole mass of blood in the body. The parts most apt to be affected with tubercle are those in which the lymphatic tissue, or, as Sanderson calls it, the adenoid structure exists naturally in the greatest abundance. The blood and the air, one or both of which may be highly charged with irritating or infecting impurities, have in the lungs the freest access to this tissue, morbidly susceptible in certain constitutions. In no other organ of the body are the irritating and the irritable bodies liable to be brought into such frequent or such close opposition as in the lungs. It is possible that the solution of the problem may be found in some such considerations; the liability of the apices to tubercular formation will be a subject for future enquiry. The greater general irritability of the lymphatic tissues in children seems to be the most plausible hypothesis in explanation of the fact that tubercle in such subjects is multiple more commonly, and to a greater degree, than in the adult. There are, it is well known, forms of acute miliary tuberculosis in which the lungs are almost unaffected, and there are even cases in which the profuse eruption of miliary tubercle is confined to one lung, but the cases in which the miliary tubercles are not found in the greatest number in the lungs form only a very small proportion of the whole number. The exceptions are usually those of rapidly fatal affections of the pia mater, a structure which is, like the lungs, singularly rich in perivascular lymphatic tissue. It is not to be concluded that any and every irritation of the adenoid tissue in the lungs is necessarily competent to produce tubercle, or that every overgrowth of adenoid tissue is necessarily possessed of the vital characteristics of tubercle. Such an exaggeration of the lymphoid nature of tubercle would only lead to a fresh series of errors such as have already more than once gravely compromised progress in the pathology of phthisis.

But to return, for the last time, to the histological aspect of the question; the distinctive anatomical feature of phthisis, whether acute or chronic, whether diffused or in the circumscribed form of grey granulations, the one common feature in the whole class of phthisical lungs, in addition to pneumonic and fibroid changes, is, as described by Wilson Fox, the

constant growth of small cells or nuclei; in some cases these are imbedded in a fine reticulum, while in others this reticulum may be less apparent, but in all the cells or nuclei are densely massed, and of the same character; this growth is attended by the destruction of the capillary circulation, and has a tendency both itself to undergo caseous change, and also by the arrest of the circulation, to give rise to this change in the tissue in which it has occurred; in some parts this growth forms round masses, corresponding to the grey granulations in serous membranes; in others it is diffused through large tracts of the tissue of the alveolar walls and bronchioles; in the latter case it is usually mingled with pneumonic products, and in a very large proportion of what appear to the naked eye as granulations it is also mixed with pneumonic products—that is, with epithelial proliferation in the interior of the alveoli.

The principal points to which I have been desirous to allude in this discourse, but which, I fear, I may only have made more obscure from the limited time at my disposal, are: The difficulty of defining tubercle dialectically; the causes and consequences of that difficulty; the necessity of an acquaintance with the various forms and phases of the tubercular granulation in order to recognise it in the complex structures of the lungs; the true nature of tubercular infiltration, that it is tubercular pneumonia; the seat and nature of the tubercular growth, that it is highly probable it is due to irritation of the lymphatic tissue of the lung; and, finally, the unity of phthisis as conceived by Laennec. In the next lecture I hope to deal, among other matters, with the subjects of the transmissibility of tuberculosis, and of its relations with scrofula. Important as may be the establishment of the fundamental identity of all forms of phthisis, the questions concerning its etiology are much more so. As Liebermeister has well observed, the principle of pathological anatomy is not the final one which science has to accept, and even in our day she is on the point of exchanging that principle for another, without yielding up the knowledge which it has furnished, and promises still to furnish. As in all experimental science, so also in pathology, the conviction is growing stronger that in the investigation of diseases, at all events, the idea of causality represents the last point to be reached, and accordingly the most scientific method of investigation must refer to causes, must be etiological. During the reign of pathological anatomy, while pathology and diagnosis were making rapid progress, and while proper recognition was made of all they accomplished, the reproach was often expressed, and with reason too, that the practical goal of all medical science, therapeutics, gained no direct advantage from the new discoveries, but, on the contrary,

that it was rather pressed into the background. But the etiological principle, carried out upon the ground prepared by the anatomical discoveries, promises to do much more for therapy. We shall always be learning more and more how to grasp the trouble at the root. The *indicatio causalis* will again assume a higher position in therapeutics, and we shall find more efficient remedies with which to answer it. And lastly, in many diseases, and probably in none more than in tuberculosis, we shall be able to hope for a successful prophylaxis, which may make therapeutics, in the narrower sense of the word, superfluous.—*Dublin Journal of Medical Science, July* 1877, *p.* 1.

18.—CROTON CHLORAL IN PERTUSSIS.

By Dr. A. Milson Roberts, Buckhurst Hill, Essex.

About a year ago I commenced the use of quinine in whooping-cough; I tried it extensively, and in some cases it seemed to do good, but in those fully developed it was almost invariably vomited. Having then had some experience of croton chloral in various spasmodic and nervous diseases, I commenced giving it in this complaint, and have now treated between two and three hundred cases successfully with it.

It has lately been advocated once or twice in your columns, but, on making inquiries amongst medical men of my acquaintance, I find it is by no means in general use, and that some who have tried it have already relinquished it as useless.

From what they tell me, I think the causes of its failure in their hands are two. (1) Smallness of dose. Children tolerate it remarkably well, and a child a year old will take one grain every four hours. For older people the dose does not increase in the same ratio as the age, a child from six to twelve years of age requiring two-grain doses, and an adult seldom requiring more than four grains. (2) Want of regular administration. It should be specially impressed on parents and nurses that to do good it should at first be given every four hours, *night and day*, even should the patient require waking up. At the end of a week it need only be given every four hours during the day, and at night when the patient is awake.

The worst cases usually completely yield in a fortnight. The drug does not upset the digestive organs, and by lessening the frequency and duration of the paroxysms, puts an end to troublesome epistaxis and vomiting. Sometimes the first few doses produce a feeling of irritation about the throat and fauces, but this soon passes off. I usually give it dissolved in compound tincture of cardamoms, and sweetened with glycerine.—*Lancet, June* 23, 1877, *p.* 924.

19.—ON HIS "DIRECT METHOD" OF ARTIFICIAL RESPIRATION.

By Dr. Benjamin Howard, M.A., M.R.C.S., Eng., Professor of Surgery in the University of Vermont, U.S.

To demonstrate more thoroughly the method I have now to introduce to you, I will ask this patient, who has not the slightest idea of the method in question, to be kind enough to consider himself just dragged ashore and apparently dead from drowning. For our part, instantly we rip away his wet clothing to the waist, and of it make a large, firm, solid bolster. Quickly turning his face downward, the bolster beneath the epigastrium, making that the highest point, the mouth the lowest; placing both hands upon his back immediately above the bolster, my whole weight is thrown forcibly forward, compressing the stomach and lower part of the chest between my hands and the bolster for a few seconds two or three times with very short intervals.

Thorough drainage being combined with thorough compression, the lungs, if they require it, are relieved of water; the stomach, if distended, of its surplus contents, forcible ejection making the process pretty complete. Should this effort happen to have been superfluous, no time has been lost, an efficient means of artificial respiration having by this process been already commenced.

Quickly turn the patient on his back, the bolster beneath it making again the epigastrium and anterior margins of the costal cartilages the highest point of the body, the shoulders and occiput barely resting on the ground.

Seize the patient's wrists, and having secured the utmost possible extension with them crossed behind his head, pin them to the ground with your left hand, so as to maintain it.

With the right thumb and forefinger armed with the corner of a dry pocket-handkerchief, withdraw the tip of the tongue, holding it out of the extreme right corner of the mouth. (This is the easiest, least barbarous, and firmest way of holding the tongue.) If a boy be at hand, both wrists and tongue may be confided to his care.

In this position, please observe two-thirds the entrance to the mouth is quite free, and the tongue is immovably fixed forward. The epiglottis, by this backward curvature of the neck, is precluded from pressure and partial closure from the undue flexion of the neck so frequently observed. The head, as Nélaton urged, is thoroughly dependent. The free margins of the costal cartilages are as prominent as they can be made, and there is a degree of fixed thoracic expansion not obtainable, I believe, in any other manner. The epigastrium being the highest point, the abdominal viscera, instead of embarras-

sing the movements of the diaphragm, tend to gravitate away from it.

To produce respiration, you now kneel astride the patient's hips, rest the ball of each thumb upon the corresponding costo-xyphoid ligaments, the fingers falling naturally into the lower intercostal spaces. Now, resting your elbows against your sides and using your knees as a pivot, throw the whole weight of your body slowly and steadily forward until your mouth nearly touches the mouth of the patient, and while you might slowly count one—two—three; then *suddenly*, by a final push, spring yourself back to your first erect position on your knees; remain there while you might slowly count one—two; then repeat; and so on about eight or ten times a minute. . . . Lest your applause may have arisen from a mistake after the first one or two movements, I will ask you to watch more carefully the effect of a longer repetition. [Demonstration repeated.] It is gratifying, gentlemen, to see that whereas on our very first effort the patient could not withhold an involuntary gasp, as we continue he comes increasingly in our power; and now suddenly desisting, from the farthest seats I think you plainly see, not only a pronounced heave, but successive waves of involuntary respiration which the patient cannot control.

This method is called the "direct method," because by it the few things needed to be done are, simply, done. The tongue needs holding forward—it is held; the ribs pressing—they are pressed. It is so simple, that a Harbour policeman, after a single lesson, has done it as well as I or any other always distant physician could do it. It is not fatiguing; the force employed is the weight of the operator, who remains in an easy position with alternations of complete rest. It can be practised by anybody, anywhere; before division of the funis, or after; in a bath, bed, or boat; and such adjunctive measures as friction, electricity, insufflation, or even tracheotomy, can be used simultaneously.

I have thus, gentlemen, in a very hasty and informal way, endeavoured to demonstrate the leading practical points of this subject. Trusting to your forbearance, I have tried to show you what appear to me as faults in the methods with which you are most familiar, and have submitted to your attention the "direct method," of the merits of which you will have a right to your own opinion. By this method I think there is obtained—

1st. *A clear passage through the lips to the lungs.*

2nd. *The greatest possible expansion of the thorax.*

3rd. *The greatest practicable diminution of the thorax;* the last two occurring with regular alternation and rhythm.—*Lancet, Aug.* 11, 1877, *p.* 195.

20.—ON RENAL ASTHMA.

By Dr. George Johnson, F.R.S., Senior Physician to King's College Hospital.

There are certain symptoms and results of renal disease which are more or less intimately associated with and dependent upon impediments and derangements of the circulation; and amongst these symptoms there is a peculiar form of dyspnœa, or, as it is sometimes called, *renal asthma*. During the progress of the various forms of renal degeneration which are commonly included under the generic term Bright's disease, a more or less persistent dyspnœa may result from a variety of causes, such as anæmia with a deficiency of the oxygen-carrying hæmatine; bronchitis or œdema of the lungs; fluid in the pleura, or in the pericardium, or in the abdomen; an enfeebled condition of the heart, either with or without valvular disease. But the dyspnœa to which I now refer is of a different character. It is not persistent, but occasional and intermittent, coming on in paroxysms, in some cases soon after food has been taken, and especially after an evening meal; while in other instances it occurs mainly during the night, when it entirely prevents or greatly disturbs the patient's sleep. In some cases the attack resembles one of spasmodic asthma, and there are loud sibilant râles over the lungs, apparently the result of bronchial spasm; but in the more typical cases the phenomena are quite different. The breathing is hurried and laborious, the heart's action is rapid and feeble, and there is more or less lividity of the lips and of the general surface; yet, on auscultation, loud puerile respiration, unmixed with râles or crepitation, may be heard over the whole of both lungs; while in other cases, especially after a long continuance of the dyspnœa, fine crepitation may be heard over the bases of the lungs, but there is obviously no deficiency of respired air, nor any change in the physical condition of the air-passages to explain the distressing symptoms. It can scarcely be doubted that the symptoms in question are the result of a form of uræmia; and they occur with especial frequency in the advanced stages of the contracted granular kidney, with which other forms of uræmic poisoning are so commonly associated.

The question then arises, What is the proximate cause of this uræmic dyspnœa? We know that dyspnœa—a distressing sense of suffocation—arises when, from any cause, the free intermingling of the blood with the respired air is prevented. The respiratory process may be interfered with alike either by an obstruction in the air-tubes hindering the access of air to the pulmonary cells, or by some impediment to the onward flow of blood through the pulmonary capillaries. A plug in

the pulmonary artery causes a sense of suffocation as urgent and distressing as an obstruction in the larynx, trachea, or bronchial tubes. I have on several occasions had the opportunity of observing the symptoms which result from an embolon or a thrombus in the pulmonary artery. When the obstruction of the artery is sudden and complete, there is usually a combination of pallor with lividity of the surface, the result of systemic venous fulness, epileptiform convulsions consequent on sudden extreme arterial anæmia of the brain, and death within a few seconds or minutes from arrest of the circulation. When the obstruction of the pulmonary artery is less complete, but permanent, and sufficient to destroy life, the respiration is hurried and laborious, and attended with a distressing feeling of want of breath. One poor girl, who was dying from this cause, panted out the statement, "I feel as if I could not get breath enough;" yet over the whole of both lungs, except over two limited patches where some blood had become impacted in the terminal bronchi, there were unimpaired resonance and loud puerile respiration. The surface of the body in these cases becomes cold and pallid, with more or less lividity of the lips; and the pulse is small and feeble, or even quite imperceptible.

It is obvious that the blood-current through the lungs is the channel by which oxygen reaches the tissues; and, when that current is obstructed, the sense of breathlessness is the result of a call from the tissues for the indispensable vitalising oxygen.

Now, it has occurred to me that the paroxysmal dyspnœa of Bright's disease may be, in part at least, explained by an interrupted circulation through the pulmonary capillaries occasioned by spasm of the pulmonary arterioles, which are stimulated to this excessive contraction by the influence of the impure blood upon the vaso-motor nerves and centre. There are some facts and arguments which support this theory of the pathology of renal asthma.

The panting, laborious mode of breathing and the sense of breathlessness are very like the symptoms which result from a plug narrowing but not completely obstructing the pulmonary artery. A consideration of the *juvantia* and *lædentia* lends support to the theory of arterial spasm of the lungs. In some cases the dyspnœa is speedily and greatly relieved for the time by inhaling the vapour of nitrite of amyl; and this relief is probably due to the well-known influence of this agent in relaxing the muscular arterioles. In many instances, relief is afforded by moderate doses of chloral hydrate, either alone or in combination with bromide of potassium. Ten grains of each of these compounds may be given twice or thrice in the twenty-

four hours. This combination probably acts by lessening the reflex excitability of the vaso-motor centre. If, however, the secretion of urine be very scanty, and the blood consequently much contaminated by retained excreta, the chloral may fail to relieve, and it may even add to the distress. In this condition, the breathing is often most effectually relieved by means directed towards the purification of the blood by promoting the free action of the skin and bowels, while the diet is carefully regulated both as regards quantity and quality, an exclusive milk diet being often the most suitable. The dyspnœa is almost invariably made worse by opium in any form; and the probable explanation of this is, that opiates, by checking the secretions of the bowels and kidneys, increase the blood-contamination, and thus intensify the exciting cause of the dyspnœa.

My late distinguished friend and colleague Sir William Fergusson often discussed with Sir George Burrows and me the etiology and treatment of this distressing symptom, from which he himself suffered severely during the last few weeks of his life. He asked me if I could explain the dyspnœa, and I suggested to him the explanation which I have just now given. He said that my explanation appeared to be in accordance with his own sensation, which was that of an obstruction somewhere within the chest; and, as the obstruction was evidently not in the air-passages nor in any of the heart's orifices, it seemed probable that it might be in the pulmonary arteries. He several times described to me an interesting observation which he had repeatedly made upon himself. He found that, by voluntarily increasing to the utmost the respiratory movements —that is, making forcible inspirations and expirations fifteen or twenty times in succession—he could for a time free himself almost entirely from the distressing sensation of breathlessness. Then gradually, in the course of a minute or two after the cessation of the forced respirations, the former feeling of want of air returned; and he explained the temporary relief by suggesting that the deep inspirations had the effect of sending on a larger volume of blood highly charged with air.

Another phenomenon closely allied to this is the œdema of the sound lung, which occurs not unfrequently when one lung has been suddenly consolidated by pneumonia or compressed by a rapid and copious pleuritic effusion. In such a case, moist crepitating sounds may often be heard in the bronchi of the healthy lung; a condition of things which must always be looked upon with anxiety, because an increase of this œdematous effusion into the bronchi may cause a fatal apnœa. There can, I think, be no doubt that the bronchial, and not the pulmonary, vessels are the source of this serous exudation. In consequence of the impervious condition of the inflamed lung, there is a

state of partial apnœa; more blood is sent to the sound lung than can be fully aërated; its progress is checked and regulated by the stop-cock action of the pulmonary arterioles; the right cavities of the heart and the systemic veins are engorged; and with this there is bronchial venous and capillary congestion, and a consequent passive serous exudation into the bronchi. In the condition here described, a timely venesection may sometimes rescue the patient from impending suffocation. The abstraction of blood relieves the distension of the veins and the right cavities of the heart, and at the same time it lessens the work of the lung in proportion as the volume of blood to be aërated is diminished.

A case of tricuspid regurgitation, which came under my care some years ago at the hospital, affords an instructive illustration of the effect of an impediment originating at the right side of the heart acting backwards, first upon the bronchial, and secondly upon the pulmonary, circulation. A woman, aged fifty-two, was admitted with general dropsy, albuminuria, a systolic bellows sound at the bottom of the sternum (*i.e.*, over the tricuspid valve), distended and pulsating jugulars, and the physical signs of bronchitis. A single dose of elaterium, acting very freely upon the bowels, removed at once the dropsy, the albuminuria, and the bronchial râles. After a few days, however, all the symptoms returned, and the patient soon died. We found, as we had expected, dilatation of the tricuspid orifice, so great that the valve was incompetent to close it; while all the other valves and orifices were normal. The lungs were much engorged. In this case, it is evident that the primary cause of all the symptoms was incompetence of the tricuspid valve. There was consequently reflux of blood into the systemic veins, renal congestion and albuminuria, anasarca, bronchial venous and capillary congestion with a muco-serous exudation into the bronchi, and then a secondary obstruction in the pulmonary capillaries consequent on the pressure of the gravitating bronchial secretion.

It is interesting to note that, on one occasion before her admission, she had spat some blood, the source of which was, in all probability, the over-gorged bronchial capillaries. We are familiar enough with hæmoptysis as a result of mitral constriction or incompetence, the source of such hæmoptysis being, in most instances, the pulmonary vessels; but disease on the right side of the heart obviously throws back the strain and pressure primarily and chiefly upon the bronchial vessels.—*Medical Times and Gazette, May* 19, 1877, *p.* 534.

DISEASES OF THE ORGANS OF DIGESTION.

21.—ON ATROPHY OF THE STOMACH.

By Dr. Samuel Fenwick, Physician to, and Lecturer on Medicine at, the London Hospital.

It is not difficult to see why atrophy of the peptic glands should have escaped the general notice of practitioners. In the case of the liver and kidney the secreting portion is so agglomerated together that any change in texture, size, or shape, is easily remarked, whilst in the stomach the glandular structure is closely intermixed with the other constituents of the mucous membrane, and forms so thin a layer that it is difficult to appreciate alterations from its normal state.

Dr. Handfield Jones has the merit of having first drawn attention to the morbid changes to which the glandular structures of the stomach are liable. He showed that the secreting tubes frequently undergo atrophy, and are replaced by a fibroid formation. The subsequent researches of Dr. Wilson Fox and Dr. Habershon in this country, and of other pathologists abroad, have amply confirmed the truth of his observations. A few years ago I directed the attention of the profession to the frequency with which atrophy of the stomach occurs in the bodies of those who have died from certain forms of cancer, and in 1870 I published a case in the Lancet in which I had diagnosed this condition, and confirmed the truth of the diagnosis by microscopical examination of the stomach after death. I purpose to lay before you, in the first place, a class of cases in which gastric atrophy occurs independently of other disease; secondly, where it occurs as an accompaniment of cancer of various organs of the body; and, thirdly, where it is only partial, or is combined with other structural alterations of the stomach.

1. *Gastric Atrophy occurring independently of other disease.*—Everyone who has been engaged for some years in the practice of the profession must have met with cases of slow, progressive anæmia, unaccompanied by emaciation, occurring in persons of middle age, unaffected by treatment, terminating in death, in which he has been unable to discover any sufficient cause to account for the bloodlessness. Dr. Addison had apparently this disease in view when he states that "for a long period I had from time to time met with a very remarkable form of general anæmia, occurring without any discoverable cause whatever—cases in which there had been no previous loss of blood, no exhausting diarrhœa, no chlorosis, no purpura, no renal, splenic, miasmatic, glandular, strumous, or malignant disease." The result of his enquiries was that he "stumbled

upon the curious facts connected with disease of the supra-renal capsules." Nevertheless, as the editors of his papers published by the New Sydenham Society point out, "anæmia" and "melasma Addisonii" are not pathologically connected; for, whilst in the one case the patient is pale, flabby, breathless, and perhaps fat, he is in the other spare, of a brownish hue, manifesting a good colour in his lips and muscles, so that his condition is one rather of asthenia than anæmia. In the four following cases, which I have grouped together, it will be remarked that the symptoms are those of anæmia, not of asthenia, and that in all a well-marked lesion of the glandular structure of the stomach was discovered after death capable of accounting for the deficiency of blood exhibited during the life of the patients.

Case 1.—A gentleman, forty-five years of age, had been ill for about eighteen months, but his symptoms had come on so gradually that it was difficult to fix the exact time when he had felt in perfect health. He complained of great weakness and inability for mental or bodily exertion. Occasionally he had pain in the back and numbness of the legs, but there was no loss of feeling nor any appearance of paralysis. He suffered from breathlessness and palpitation on the least exertion. He was not emaciated, but his face was of that pale-yellowish colour so often seen in persons affected with malignant disease; and the lips, tongue, and throat were bloodless. His appetite was exceedingly bad; he suffered from flatulence, and occasionally from bilious vomiting, and the bowels were much confined. The pulse was unusually small and feeble. He was unable to assign any cause for his complaint. There was no history of phthisis or cancer in his family; he had lived temperately, and had always enjoyed excellent health until his present illness. On careful examination, no dark patches could be discovered either in the mouth or on the skin; the heart and lungs appeared to be perfectly normal; the liver and spleen were of the usual size; there was no enlargement of the thyroid or of the lymphatic glands; the stomach was not dilated, and no abdominal tumour could be detected. The urine was clear, acid, and free from albumen and sugar. A drop of blood taken from his finger, when examined by the microscope, showed no increase, but rather a diminution, in the number of the white globules. I prescribed steel and quinine with a small dose of cod-liver oil, and recommended a nutritive diet with a moderate allowance of wine.

About a week after I saw him I learned that he had been attacked with severe and persistent vomiting. Some of the fluid rejected from the stomach was sent me, but, on examination, it proved to be only bile. After a purgative the vomiting

ceased. When he again visited me he looked pale and more feeble than before. The pulse was now so small that it could be scarcely felt, and I was unable to obtain a sphygmographic tracing. He complained of a little pain below the clavicle, but in other respects his symptoms were unchanged. His appetite was exceedingly bad, and he was distressed with flatulence. After this he gradually became more feeble and anæmic, and at last he sank from exhaustion, being delirious two or three days before his death.

Post-mortem examination.—The skin was of a pale-yellow colour, and there was no dark discolouration; there was a large amount of fat in the subcutaneous tissue; a considerable quantity of fluid was found in the left pleura, and both lungs were rather œdematous, but, excepting a slight grittiness in each upper lobe, there was no other disease. The heart was covered by a layer of fat, but was otherwise normal; its cavities were unusually empty, only a small, soft, gelatinous clot being present in the right ventricle. The liver, spleen, pancreas, and supra-renal capsules were all normal. One kidney seemed somewhat congested, but was healthy in its structure. The stomach was empty, excepting a small quantity of gas, and *it showed no signs of post-mortem solution.* When small portions were placed beneath the microscope, the depressions upon its surface were seen to be well defined and rather larger than usual. The whole of the glandular structure of the organ was in a state of atrophy, and in no part could I succeed in obtaining a specimen of perfectly normal structure. In the pyloric and middle regions the secreting tubes seemed to be converted into a mass of fibrous tissue, and it was only near the cardiac end that a trace of gland-structure could be observed. In this situation the gastric tubes were represented by scattered flask-like bodies filled with granular matters and fatty epithelial cells. In other places the ends of the tubes were expanded into the form of cysts. Each of these was surrounded by fibres, and was lined internally by a layer of cells, the contents consisting of fatty cells and of granular matters. The villi of the upper portion of the intestine were large, prominent, and contained fat. The fat, however, was not in the form of emulsion, but appeared in large drops on the interior of the villi. After the microscopical examination was concluded, I scraped off the mucous membrane of the stomach, and made an infusion of it with distilled water. To two ounces of this infusion was added half a drachm of hydrochloric acid. A cube of hard-boiled albumen of egg was suspended in this mixture and was digested in a water-bath at blood-heat for nine hours. At the end of this period the albumen was slightly softened on the surface, but its weight was not lessened.

The first symptoms of atrophy of the stomach show themselves very gradually; so much so that most of the subjects of the disease are unable to state exactly when the earliest signs of ill-health attracted their attention. A slight loss of strength, and a failure of appetite as regards animal food, especially when the meat is underdone, are usually the earliest indications. At this time flatulence is the only sign of indigestion, with the exception, perhaps, of slight constipation. At a later period the appearance of the patient is very striking. The face is of a pale-yellow colour, the lips and throat are intensely white, and he looks as though he were suffering from chronic Bright's disease or some malignant tumour. With the exception of Case 3, there was in none of the instances any marked loss of weight. Most of them were stout, one or two unusually so. The appetite for animal food is now exceedingly bad, the thought of it often exciting a feeling of loathing; but nourishment of a starchy nature is readily taken. Some are still able to eat fish or chicken; others object even to these, but can swallow soups. Occasional vomiting comes on; others suffer from nausea. Flatulence is almost always a subject of complaint, and attacks of diarrhœa replace the former constipation. The strength is greatly reduced, and the loss of memory and incapability for mental exertion force themselves on the sufferer's attention. The least exertion induces palpitation and dyspnœa, which are often so distressing that the patient is forced to keep in the recumbent position. The pulse is small and thready, but not necessarily quickened. The progress of the case is generally slow, and as time goes on the feebleness and anæmia increase from week to week. Some complain of severe pains of the limbs on the slightest exertion, and the pains are stated to be as if they had undergone an immoderate amount of muscular effort. As death approaches, dropsy of the limbs sometimes shows itself. or effusion may take place into one or both pleuræ. Some complain of tenderness over the epigastrium; others suffer from vomiting or uncontrollable diarrhœa; whilst others become delirious, and sink gradually into a state of coma.

There are no physical signs peculiar to the disease. On account of the anæmia you commonly find systolic murmurs over the heart, carotid arteries, and jugular veins. The stomach presents no evidence of tumour or dilatation, and there is no enlargement either of the lymphatic glands or spleen. The urine is of low specific gravity, contains only a very small amount of urea, but neither albumen nor sugar is present in it. The blood is thin and watery, and under the microscope shows no increase, but rather a diminution, in the numbers of the white corpuscles. Both the white and red cells appear to be normal in size and shape.

I have already pointed out that it was in the search for the cause of "idiopathic anæmia" that Dr. Addison lighted upon the disease of the supra-renal capsules now distinguished by his name. It has been conjectured that in this malady there may be some atrophy of the stomach, as many of its most prominent symptoms point to gastric disorder. A young man was admitted under my care into Fitzgerald ward two years ago with the discolouration of the skin and other symptoms of "morbus Addisonii." After death, the correctness of the diagnosis as to the disease of the supra-renal capsules was confirmed, but the most careful microscopical examination failed to show any atrophy of the peptic glands. A woman about forty-five years of age was last year under my care in Davis's ward. In addition to the discolouration of the skin, she had frequent vomiting, great loss of appetite, and extreme pain and tenderness of the epigastrium. The tenderness was so excessive, that it formed the chief subject of complaint. Five years previously she had, according to her own account, been in a very similar state, but had quite recovered her health in the Victoria-park Hospital. Now, here was a case where severe pain, extreme tenderness, and frequent vomiting pointed to gastric disorder, whilst the duration of the symptoms was so long, that anatomical changes in the stomach had ample time to pass on to the latest stages. Yet, after death, there were only the appearances ordinarily presented by chronic catarrh, and no atrophy of the secreting structures could be discovered excepting in the pyloric region. It is, then, sufficiently plain that although many of the symptoms of Addison's disease and of gastric atrophy may present points of similarity, yet these maladies are essentially different in their nature.

2. *Gastric Atrophy occurring as an accompaniment of Cancer in various organs.*—Whilst engaged in investigating the condition of different organs in the bodies of those who had died of cancer, I was struck with the frequency with which atrophy of the gastric glands presented itself. This was more especially in cases of cancer of the mamma, although the anatomical changes in the stomach, as you will shortly see, were not confined to those who had perished of malignant disease of this organ. In the earliest stage of the disease the solitary glands were enlarged and filled with cells and nuclei; the gastric tubes, and sometimes the muscular fibres, were displaced by these bodies, which were also scattered everywhere through the mucous membrane. The tubes adhered firmly to each other, but still contained normal gastric cells. At a later stage the solitary glands appeared empty in the centre, but were surrounded by thick layers of nuclei; the tubes could be no longer traced throughout their whole extent, but could be recognised only as bulbs filled

with fatty cells, or as lines of cells; whilst the whole tissue was obscured by fatty and granular matters. In the latest stage the solitary glands had disappeared, and the secreting tubes were replaced by fibres, so that no vestige of the original structure remained. In some of the other cases the solitary glands were not so much affected, but the tubes were much thickened and firmly adherent to each other, as in the earlier stages of idiopathic anæmia. The mucous membrane of the stomach was usually much wasted, but it was found to be chemically altered in some of the cases examined. In one, when dried and digested in ether, the tissue lost as much as 32·8 per cent.; whilst the average amount removed by ether in five cases, where death had occurred from other causes, independent of cancer, was only 12·6 per cent. In another of the cancer cases, although the microscope did not detect any anatomical changes, the amount of fatty matter contained in the mucous membrane was upwards of 19 per cent. As might be expected, the amount of pepsin present in the gland structure was much lessened where atrophy was present. In one case, ten grains of albumen were digested for eleven hours in an acidulated infusion of the stomach, and only six-tenths of a grain were dissolved; whereas the average amount of albumen dissolved in similar experiments conducted with the stomachs of persons who had died of other diseases amounted to four grains. In another cancer case, although the microscope detected no anatomical changes, the albumen had gained by imbibition one grain in weight, and was merely softened at the edges.

It must, however, be borne in mind that the atrophy of the stomach does not occur in every case of cancer of the breast: in four out of fifteen, no anatomical changes could be discovered; in three of these the mucous membrane was very thin, but in two of them, in which artificial digestion was tried, the usual amount of coagulated albumen was dissolved.

You will doubtless remark that, although in this, as in the preceding class of cases, the secreting structure was found to be in a state of atrophy, yet that the solitary glands were more generally enlarged where cancer of the breast was present than in "idiopathic anæmia." This change in the solitary glands is not necessarily connected with atrophy, but may be present, more especially in the pyloric region, in any case of chronic inflammation of the gastric mucous membrane. In one case of cancer of the breast, these bodies were so generally enlarged that the cardiac end of the stomach was everywhere sprinkled over with small white spots about the size of a pin's head, that looked, but did not feel, as if raised above the surface.

But an interesting question will here no doubt arrest your attention. What is the relation between the gastric atrophy

and the malignant disease of the breast? Is the tumour induced by the imperfect state of the digestive organs? The hypothesis might suggest itself to your minds that the atrophy of the stomach may so alter the digestive process that an imperfectly elaborated albumen might be absorbed, and thus give rise to an abnormal form of the fibrine or gelatine that so largely enters into the chemical constitution of malignant tumours. But such an idea seems to be contradicted by the fact that in "idiopathic anæmia," where there is such a decided decrease in the ordinary amount of the albuminous constituents of the blood, we meet with no malignant tumour in any organ of the body; and, again, that no gastric atrophy can be found in the majority of the cases of cancer affecting other organs than the breast. To make this more plain I have arranged the number of cases of cancer of various organs in which I examined the stomach microscopically in a tabular form.

	Stomach atrophied.	Total number of cases.
Cancer of breast	11	15
,, of stomach	5	5
,, of uterus	3	24
,, of tongue	1	3
,, of rectum	1	2
,, of groin, penis and bladder	0	3
,, of glands, bones, skin, and lungs ..	0	8
	21	60

We can scarcely use the cases of cancer of the stomach for the purpose of drawing any conclusion as to the probability of the glandular atrophy tending to the production of malignant tumour, because in all cancers traces of the diseased action extend far beyond the apparent limits of the new growth. But I think I have observed that the nature of the microscopical changes varies with the characters of the tumour. That where the latter is scirrhus, the atrophy of the gastric gland-structure is much more decided, and accompanied by an increased fibrous formation between and below the tubes; whilst this is not the case, at any rate to the same extent, in the softer kinds of malignant growths. You will remember that I pointed out to you, when speaking of cancer of the stomach, that the coexisting atrophy of the gland-structures was a point of considerable practical importance, as it does not occur in simple ulcer, and that to its presence we must attribute much of the loss of appetite and flesh and the rapid anæmia that accompany malignant disease of this organ.

But the above table shows that the occurrence of atrophy of the stomach in cancer of the breast is not a mere accident, for eleven out of fifteen presented it in a greater or less degree, whilst it was found in only three out of twenty-four instances of cancer of the uterus, and in none of the cases where the malignant disease affected the glands, bones, skin, or lungs. I have seen it most developed where the tumour of the mamma was of a hard fibrous character, where the disease was of slow growth, and where eventually small nodules formed in the skin. If we admit that these nodules arise from an affection of the lymphatics, then the co-existing enlargement of the solitary glands of the stomach of a *non-cancerous* character is interesting, as pointing to the probability of an early and a general morbid condition of the lymphatic structures. The relation between gastric atrophy and cancer of the breast I believe to be merely this: that scirrhus of the mamma generally takes place in persons in whom other organs are simultaneously undergoing fibroid degeneration; and I think, in all probability, it will be found that the atrophy is so much more common in cancer of the breast and pylorus, because these organs are more generally than others the seat of the more fibrous forms of malignant disease. Why the stomach should be more affected than the liver or kidneys I am unable to explain; but you cannot fail to be struck with the common occurrence of emphysema of the lungs, thickening of the valves of the heart, and other general fibroid changes in persons who have perished from scirrhus of the breast or other organs.

You may fairly ask, why should scirrhus be accompanied by degenerative changes in other organs, when they are absent in the other forms of malignant growths? Now, you must remember that fibroid degenerations produce anatomical changes perceptible by the microscope, but that there may be—indeed, there are—degenerations of tissue which require chemical and not optical means of investigation for their detection. In some of the cases of cancer of the uterus, for example, although the mucous membrane of the stomach was not lessened in bulk, the tubes seemed bathed in an albuminous fluid, in others the membrane cut as if it were composed of gelatine, and in only a few instances did spirit or chromic acid harden the membrane, as is the case where death has occurred from disease of a non-malignant character.

The chemical composition appeared to vary; in one case the softness seemed to arise from fatty degeneration, as ether removed 33·5 per cent.; in another 50 per cent. was composed of albumen; in a third, along with a small proportion of fat and albumen, there was a great excess of gelatine. In two cases of cancer of the uterus, fatty degeneration of the

mucous membrane of the intestines was present; in some the intestinal tubes were diseased, and only the remains of their closed and bulbous ends could be discovered. Taking the whole cases of cancer together, in eighteen out of nineteen instances in which the gastric tubes were normal, morbid changes of some kind were detected in the intestinal canal. Then, again, in a large proportion of the cases where death had resulted from cancer of the uterus, the muscular structure of the heart presented evidences of disease. The fibres were very soft, their cross-markings faint, in some specimens entirely lost, and the structure was loaded either with fat globules or granular matters. In many cases the change seemed to me to differ from the ordinary fatty degeneration, for I was able to dissolve by boiling alcohol a fatty material that yielded lime on incineration. Now, I am quite prepared to admit that many of these chemical changes in the tissues at a distance from a malignant tumour may be the result of the local disease. The growth of many of the softer forms of cancer is, as you know, extremely rapid; but the disintegration of the structure is equally so. You may therefore readily imagine that fatty salts of lime and other products of disintegration may be reabsorbed from the portions undergoing involution, and, being incapable of elimination, may be deposited in the structures. What I wish, however, to point out to any of you who may direct your inquiries to this most important and interesting part of pathology is, the necessity for you to look away from the organ affected and endeavour to ascertain the condition of the other structures of the body. All of you know that of late years the most laborious investigations have been pursued into the minute structure of new growths. But, in a practical point of view, we are but little wiser than our predecessors, for we are as powerless as they either to prevent a cancer or to ward off its ill effects upon the constitution. If, however, future researches should prove, as I suspect they will, that certain materials are apt to be produced in the course of degenerative actions as modifications of the normal products of tissue changes, and that these materials, in virtue of their physical or chemical properties, are capable of exciting the abnormal growths we term cancers, then we should have a better chance of ascertaining what circumstances are most apt to give rise to the development of malignant disease. Or if we should discover that the "cancerous cachexia" is in many cases dependent on the absorption of disintegrated materials incapable of elimination from the body, we should have a definite aim in view, to which our experiments and observations might be usefully directed.

I have had fewer opportunities of watching the course and symptoms of atrophy of the stomach in this class of cases,

because most of them come under the care of the surgeon; but such as I have seen were very similar to those before described as "idiopathic anæmia."

3.—[Speaking of *Partial Atrophy of the Stomach, or Atrophy combined with structural changes in other parts of the organ,* Dr. Fenwick observes:—]

The pyloric is more liable to this change than the other regions, and most of the descriptions of the morbid appearances given by authors have been drawn from investigations into the structure of this part. I think, however, that the anatomical alterations so often met with near the pylorus are analogous to the senile atrophy so common in the other glandular structures of the body.

The development of cancer is associated with degeneration of the tissues. Primary cancer is most commonly met with in the breast, uterus, and pyloric end of the stomach. Now you are aware that the two former organs lose their functional powers sooner than the other glandular structures of the body, and here we see the same commonly occurs at the pylorus at an early age.

But a partial atrophy of the glandular structure of the stomach occurs as an accompaniment of an inflammatory condition of the connective tissue of the organ; and the symptoms of such cases are, of course, greatly modified, as you will see in the following.

Case 5.—A man, thirty-nine years of age, was admitted into the hospital under my care in Sept., 1873. He had enjoyed good health until thirteen years before, when he had contracted chancres, which were followed by severe secondary symptoms that had laid him up for eight or nine months. In 1871 he had severe pains in his lower extremities, and in 1872 had first observed a lump in the right axilla. For six months he had suffered pain after food and frequent vomiting of a sour, burning liquid, but had never had hæmatemesis. On admission, the lump in the axilla proved to be an enlarged gland, the size of a hen's egg, the vomiting was frequent, and he complained much of weakness. There was a systolic murmur at the third rib; on a line with the umbilicus and to the right of it there was a tender part, which gave the sensation as if there was a deeply-seated thickening. He gradually became weaker, the vomiting increased, the glands above the clavicle became enlarged, and he eventually sank very slowly from exhaustion. On post-mortem examination, the only appearance of disease was in the stomach. The pyloric region was generally thickened, hard, and leathery, but there was no tumour. The pyloric orifice was of normal size, and the stomach not dilated. Microscopically, the mucous membrane

presented everywhere atrophy of the tubes, but the morbid changes were not to the same extent as in the cases of "idiopathic anæmia" before described. The enlarged glands showed no appearances of cancer.

Now, I do not propose on the present occasion to take up your time on the question whether the thickening of the stomach was of syphilitic origin. The point of interest is, that the man should sink from exhaustion when the pylorus was not narrowed and the stomach not dilated. We can only attribute his death to the co-existing atrophy of the peptic glands and the consequent imperfection of his digestive powers. You will find cases similar to this reported by various authors, in which no disease could be found after death, excepting an inconsiderable thickening of the connective tissue of the stomach. They are generally quoted as showing how slight an amount of disease occurring in an organ necessary for the continuance of life will suffice to produce death. The acidity and vomiting where, as in this case, there is no stricture of the pylorus, seem to arise from the long retention of the food in the stomach, caused by co-existing degeneration of the muscular coat.

Atrophy of small isolated tracts of the mucous membrane of the stomach is often associated with, or results from, chronic inflammation of the connective tissue between and below the gastric tubes. One very common cause is disease of the heart. In the earlier stages the tubes are firmly united together, and the whole mucous membrane is increased in bulk; but at a later period it becomes tough and leathery, and atrophy of the glandular structures is apt to ensue. This is chiefly at the pyloric end, for at this part the effects of long-standing venous congestion are most strikingly exhibited. I need not remind you that a similar state of atrophy of the kidneys, liver, and other organs is apt to occur whenever the venous circulation is for a long time obstructed. But if venous congestion is capable of inducing atrophy, you would expect to find this condition of the stomach in cirrhosis of the liver. Such is the case, and as the earlier symptoms of cirrhosis are usually those of chronic gastritis, so you rarely meet with the stomach of one who has died of that complaint in a perfectly healthy state. Besides the cases in which there has been long-standing venous congestion, most of the instances of partial atrophy of the stomach occur in persons affected with chronic Bright's disease or with phthisis. The symptoms arising from this class of cases of atrophy are much less striking than in idiopathic anæmia or cancer. This is due to the circumstance that the extent of the glandular structure involved is so much less than in those formerly described, and also because the anæmia produced is

apt to be ascribed to the more evident co-existing visceral disorders.

Diagnosis.—It is unnecessary to say much respecting the diagnosis of the partial forms of gastric atrophy. Their existence must be determined by reference to the other diseases from which the patient is suffering—diseased heart, cirrhosis, granular kidney, or phthisis—and to the history of the symptoms of gastritis. But it is different with respect to "idiopathic anæmia." You will often find it almost impossible to come to a correct conclusion until you have watched the patient for some time. It will assist you in your diagnosis to bear in mind that the disease occurs only in middle-aged or in elderly persons, that the male is more apt to be affected than the opposite sex, that the patients, although strikingly anæmic, are commonly stout, often unusually so, that palpitation and breathlessness on exertion are prominent symptoms, that the appetite is very bad, especially as regards all kinds of animal food, and that where the female is the subject of the malady there is usually a history of scirrhus of the breast. But you must also be careful to exclude all causes likely to produce anæmia, such as chronic diarrhœa, dysentery, albuminuria, leucorrhœa, &c. Now, in practice, you meet with difficulties you would not, perhaps, have expected. Hemorrhage, for example, may be going on from some organ, and the patient may be either unaware of it, or may attempt to conceal the fact. A lady, between thirty and forty years of age, who had been looking ill for some time, but had made no definite complaint, became exceedingly weak and feeble. When I saw her she was strikingly anæmic, breathless on the least exertion, but had no pain or vomiting, nor was she aware of any discharge of blood. The urine was not albuminous. Taking into consideration that the anæmia had come on rather suddenly, I surmised that she had an ulcer of the stomach or duodenum, and an attack of hæmatemesis shortly afterwards proved the correctness of the suspicion. You know that ulcerations of the upper part of the digestive tract are ordinarily associated with pain and vomiting, but a painless ulcer in this part of the body is not so rare as is usually supposed. You may have, in fact, cancerous tumour of the stomach with only trifling pain, and in the early stage the patients may be free from any sufferings at all.

Treatment.—It is unnecessary to mention the treatment of the cases where partial atrophy of the stomach is present, because it is mainly dependent on the nature of the affection of the other organs with which it co-exists. But, as regards that of "idiopathic anæmia," and that form associated with cancer, I am sorry to say the results of treatment have been

very discouraging. Every case in which I felt tolerably confident of the correctness of the diagnosis has gone on from bad to worse, without being apparently influenced by any of the measures adopted for its relief.

Almost every patient had been treated with iron, in some form, before he came under my notice, but no satisfactory results had ensued. For a time, some thought they had derived benefit, but the anæmia soon became more decided, although the medicine was steadily continued. I have tried arsenic, quinine, strychnine, and various other tonics, with equally unsatisfactory results. Pepsin was also generally prescribed, either alone or in combination with ipecacuanha or capsicum. There is one point, however, in which you may do great harm. and that is by the use of purgatives, more especially by salines, I have seen the patient's strength rapidly reduced by their use, and I would advise you, if the bowels are very inactive, only to employ the very mildest aperients.

The want of appetite renders the subject of diet one of great difficulty. In the earlier stages the patient can usually take chicken or fish, but, as the disease increases, these must be abandoned, and you must trust to soups, milk, &c. Fat and starchy materials are often taken without difficulty, even to the last. Many of those that have come under my observation have been sent from home, under the idea that change of air andscene might produce improvement. Travelling has usually done more harm than good, and in the later stages, as was the case in one of the patients before mentioned, a journey of very moderate length may cause fatal exhaustion.—*Lancet, July* 7, 14, 21, 1877, *pp*. 1, 39, 77.

22.—ON THE HISTOLOGY OF THE SO-CALLED NUTMEG LIVER.

By Dr. J. WICKHAM LEGG, Senior Casualty Physician and Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital.

If the liver of a healthy animal be examined immediately after death it will be found to present a uniform red-brown appearance. Soon after, however, the uniform appearance is lost. The surface becomes mottled, and the mottling on further examination will be found to correspond closely with the distribution of the acini of the liver. The circumference of the acinus is pale, while the centre is deeper coloured. The cause of this unequal distribution of colour is to be found in the arrangements of the blood-vessels ; the portal vein, which within the liver closely resembles an artery, is distributed to the circumference of the lobule, and, like an artery, empties

itself of blood soon after death. The hepatic vein, on the other hand, which arises in the centre of the lobule, remains full of blood, as is the rule with the ordinary systemic veins.

It is an exaggeration of this mottling to which the name of nutmeg liver has been given. Whenever there exists a hindrance to the return of blood from the hepatic vein, this state of the liver may very readily be set up. The radicles of the hepatic vein become gorged with blood, and a deep brown-red colour is thus given to the centre of the lobule; while the outer part of the lobule, to which the portal vein and hepatic artery are supplied, remains pale. Hence a deep contrast between the centre and circumference of the same lobule.

Kiernan described nutmeg liver under the name of the second stage of congestion, or of active congestion of the liver. "It very commonly attends," he says, "disease of the heart and lungs; the congestion begins in the hepatic veins, and extends towards the portal veins." He has thus accurately enough described the pathology of the disease, so far as it relates to the blood-vessels; before him it was well-nigh impossible to be explained.

Cirrhosis of the liver and nutmeg liver have not always been distinguished. It is clear that five-and-thirty years ago the two morbid states were confounded. Becquerel, in analysing his cases of cirrhosis, sets down heart disease as a cause in exactly one half. So more recent writers, as Rokitansky and August Foerster, have mentioned heart and lung disease amongst the causes of true cirrhosis.

Budd appears to have been one of the first to separate cirrhosis and nutmeg liver, and on the ground that simple passive congestion of the organ could never lead to an active inflammation. Dr. Handfield Jones a few years later expressed his opinion that nutmeg liver does not terminate in the contracted hobnail cirrhosis. As this writer's description of the state of the connective tissue in nutmeg liver seems to me to agree more closely with the facts of the case than those of many other writers, it may be worth while to detail his views more at length. Dr. Handfield Jones regards, as the essential circumstance of the nutmeg liver, the effusion of an unhealthy plasma, not only in the canals and fissures of the liver, that is, in the capsule of Glisson, but in the external part of the lobules. The investing membrane of the lobules is very greatly increased, and becomes much more condensed and more distinctly fibrous. The effused plasma likewise seems to insinuate itself between the cells in the external part of the lobule. With the description of the changes in the lobule itself I am less able to agree. Liebermeister was, I believe, the first to acknowledge that Dr. Jones' view of the changes in the capsule

of Glisson was correct, and he has devoted a long chapter in his book on diseases of the liver to this subject. But, so far as I know, few writers have followed in his steps, Dr. Henry Green being one of the few who describes an inter-lobular growth as being constantly seen in nutmeg liver. Rindfleisch and Klebs certainly speak of an interlobular growth, as being sometimes seen, but they evidently look upon it as an accident not to be associated with the essential phenomena of nutmeg liver.

The prevailing doctrines as to the histology of nutmeg liver are, however, altogether different from that of Dr. Handfield Jones. They are those expressed by such authorities as Virchow, Frerichs, and Rokitansky, and may be stated as follows: that the cells of the liver in the centre of the lobules atrophy from the pressure of the dilated vessels; that their place in the centre of the lobules is then taken by a highly vascular tissue, formed of capillaries and new connective tissue, while the cells of the circumference of the lobules and the inter-lobular connective tissue remain unaltered. My observations have led me, however, to take a very different view of the disease, and have caused me to support in general the statements of Dr. Handfield Jones and Professor Liebermeister.

The observations which follow were made upon twenty livers which showed a well-marked nutmeg appearance, met with in the post-mortem room of St. Bartholomew's Hospital during the last two years. Parts of the livers were either hardened altogether in weak chromic acid, or first kept for a few days in weak chromic acid and then put into spirit of wine. The thin sections were in all cases coloured with carmine and mounted in glycerine. The following descriptions must be understood of sections examined with Hartnack's microscope, ocular 3, objective 9, *à immersion:*

In the earlier stage of the disease the chief morbid appearance is to be found in the centre of the acinus, and is caused by a dilatation of the radicles of the hepatic vein. In the section these vessels may be seen distinctly enlarged and still completely filled with red corpuscles. Their appearance varies according to the direction of the section of the blood-vessels. When the cut has fallen lengthwise upon the vessels, they are seen as long channels passing between the liver-cells; when the vessels are cut directly across, they are then surrounded by a ring of liver-cells varying in number. The liver-cells themselves seem to be at first not much changed in character. Their outline is, however, hard to make out, so that they cannot be readily counted; they show a large nucleus and granular contents.

In the later stages the dilatation of the capillaries becomes

very great. The vessels in the centre of the acini are dilated to twice or three times their natural size, and in this way grave changes are wrought upon the tissues immediately around them. The rows of liver-cells between the vessels, at first only somewhat lengthened and flattened, become so much pressed upon that their shape becomes almost linear; a nucleus can scarcely be made out, and their contents are seen to be dark and granular.

The latest stage of all is that in which the centre of the lobule becomes little more than a network of vascular tissue, between the meshes of which a few highly granular and pigmented liver-cells lie. On looking at specimens in this stage with a low power (Hartnack, oc. 3, obj. 4), the outer part of the acinus is seen to be formed of a ring of almost colourless liver-cells, while the centre is filled with a red tissue, studded with pigmented liver-cells; and this tissue, as seen with a low power, looks very like connective tissue. But on examining the specimen with higher powers, this tissue is found to be made up of channels, the walls of which are exceedingly delicate and transparent, and within which red blood-corpuscles can yet be made out. No nuclei can be seen in the midst of this tissue as in the interlobular tissue, but studding the field are numerous rounded or slightly polygonal cells, with highly pigmented contents, and containing no discoverable nucleus. These cells I hold to be the remains of liver-cells.

This last stage of the disease is rarely met with, but it is clear that it is this stage which Rokitansky has endeavoured to portray in the woodcut which shows an intra-lobular vessel surrounded by a highly-nucleated connective tissue, in which a few pigmented liver-cells are seen. I am quite unable, however, to agree with the statement that the tissue in the centre of the lobule is an overgrowth of the connective tissue. It is rather to be regarded as the transparent wall of the vessels. The only other change which I have been able to make out is a thickening of the wall of the hepatic vein in the centre of the lobule.

These changes in the vessels and cells rarely go beyond the middle zone of the acinus; I have not found them exceed this in any of the specimens which I have examined. Fatty infiltration of the cells of the outer border of the lobule is not so common or so usual as from the writings of others I had been led to believe. In a very few only did the amount of fat in these cells exceed what is natural. In the few cases in which there was more fat than is common the excess was very great, and the border of the acinus showed cells filled with large fat drops for some depth towards the centre.

The connective tissue of the capsule of Glisson is the seat of

important changes. The centre of the acinus is not the only part of the liver which suffers; a large part of the connective tissue of the liver becomes actively engaged in the disease. In the earlier stage the capsule of Glisson may be found increased in size and with numerous lymphoid corpuscles present in it. This may be seen very early. In a child of eight years, in whom, so far as could be made out, the heart disease has lasted only three weeks, a considerable increase of the connective tissue of the liver was observed, and the connective tissue itself was studded with lymphoid bodies. In the earlier stage of the disease this appearance, however, is not always met with. The connective tissue may be increased in amount, but no lymphoid corpuscles seen among the fibres. When these are present they are most abundant in the sheath of the vessels; and it is around the interlobular vessels that the increase of the connective tissue is most marked. There can then be no doubt as to activity of the process going on in the connective tissue of the liver, but if these lymphoid bodies be absent it is a matter of some nicety, requiring considerable experience in the histology of the natural liver, to decide if an increase of the connective tissue have really taken place,

In the later stages the increase of the connective tissue becomes far more marked, and the lymphoid bodies are never absent throughout the whole of the liver. At least, I have always been able to see them in some part of the liver in all the cases of advanced nutmeg liver which I have examined. The connective tissue is strewn with them just as in primary cirrhosis. They are often very abundant indeed; but they do not show any linear arrangement as in cirrhosis, but are scattered irregularly through the tissue. This lack of arrangement may be due to the fact that but a small piece of the connective tissue is visible at one time under the microscope, so that it is well-nigh impossible to follow for any long distance the same tract of overgrown connective tissue.

The connective tissue is not always developed to the same degree in every part of the same liver. Often quite close to spots where the interlobular tissue is actively engaged, crowded with lymphatic corpuscles, and much increased, a spot may be found quite free from these lymphatic bodies, and seemingly not greatly increased in bulk. This is an exceedingly common appearance in nutmeg liver, and one for which the observer must be prepared in forming a judgment upon it.

I have not found the connective tissue in any of the specimens which I have examined so greatly increased as to penetrate within the lobules themselves, although I can well conceive such an appearance in far advanced specimens of the disease to which the name of atrophous nutmeg liver has been

given. I must still, however, be allowed to doubt, with Dr. Handfield Jones, if the growth of the interlobular tissue ever becomes so great as to give a hobnailed appearance to the liver.

It has been laid down as a pathological law by Sir William Jenner that, whenever an organ is mechanically congested for any length of time, the part becomes indurated, and also that an overgrowth of connective tissue takes place, although this is rather a matter of inference from Sir William's words than a direct statement of his. He found in cases of congestion of the heart that the interstitial tissue lying between the muscular fibres was increased. It is also well known that one of the results of long-continued mechanical congestion of the kidney is an overgrowth of the intertubular connective tissue. It is not, therefore, surprising to find like changes in the interlobular tissue of the liver in mechanical congestion of that organ. The overgrowth cannot be due, as Rindfleisch implies, to a mere coincidence, as I have found it in almost all the specimens which I have examined, even in the earliest stage of the disease; nor can it be merely an early stage of cirrhosis caused by habits of intemperance, for I have found the same interlobular overgrowth in four nutmeg livers of children varying from eight to twelve years in age, and who were unlikely to have been of intemperate habits. The presence of the lymphoid corpuscles I look upon only as evidence of the activity of the interlobular growth, and in no way related to a chronic inflammation.

The shrinking in the after stages of nutmeg liver is, in my opinion, due to the same cause as in cirrhosis, that is, to an overgrowth in the capsule of Glisson. A decrease in size cannot be caused by an atrophy of the parenchyma of the liver, so long as this atrophy is commensurate with the pressure upon the parenchyma by the dilating blood-vessels, for the decrease in size of the cells will be exactly counterbalanced by the increase in size of the blood-vessels. According to many authors, a new growth springs up in the midst of these dilated vessels, and it is by the growth of this new tissue that the decrease in size of the liver is brought about. I think this is but an imperfect explanation of the decrease in size, and it would seem more probable that if there be an interlobular growth, that the shrinking of the liver is due to the same cause as in cirrhosis.

In conclusion, I should like to add a few words as to the name of red atrophy which has been given to the atrophous nutmeg liver by Rindfleisch. Nothing can be more confusing than to have the same name given to two different states. The name of red atrophy was years ago given by Virchow to a rare state of liver met with after wasting diseases, such as typhoid.

This name has been accepted in this sense by pathologists such as Rokitansky and Foerster, and it is scarcely worth while to introduce a disturbing element into the nomenclature when the name of atrophous nutmeg liver is sufficiently good, and no advantage can be attained by change. — *Medico-Chirurgical Transactions, vol.* lviii., *p.* 345.

DISEASES OF THE URINARY ORGANS.

23.—ON THE URINE OF ACUTE RHEUMATISM WHEN TREATED WITH ALKALIES AND QUININE.

By Dr. A. W. Barclay, Physician to St. George's Hospital.

My attention was first arrested by the rapidity by which urine previously loaded with phosphates became perfectly transparent, without any diminution of the quantity of alkali given, when two or three grains of quinine were administered with each dose of the medicine. At the same time, cases often presented themselves in which the urine continued phosphatic, in spite of its employment. The effect of alkali on the urine is dependent on the amount of acid secreted and presumably present in the blood. In some instances a few doses will produce alkalescence, in others it is more slowly established, and in exceptional cases very large quantities must be given before the alkaline reaction is observed. That this should be so is not surprising when we consider the wide limits within which the symptoms of the disease vary in different individuals, and the varying susceptibility of each to the action of any given remedy. It may or may not be that the degree of acidity of the urine is to be taken as an index of the intensity of the attack and the quantity of alkali which ought to be administered; but certainly, in my own experience, the number of joints affected, and the severity of the inflammation in each, utterly fail to indicate the amount of alkali necessary to render the urine phosphatic. If there were any ready method of determining the acidity of the urine, we should perhaps know better how and when to give the alkali; but the knowledge would be of no great value, as we can so readily watch the effect of its administration. If the attack be very acute, and the liability to cardiac complication appear to be great, it seems best to give the remedy at first rather in excess of the requirements, and modify our treatment as soon as the test-paper shows that the urine is alkaline. It is not unusual in this hospital to prescribe a drachm of bicarbonate of potash every four hours in the ordinary saline draught of the hospital, which contains half a drachm more neutralised with citric acid. This treatment may have to be persevered in for several days before the phosphates are precipitated. There is a very general impression that large doses of alkalies have a lowering

effect on the system, and it has been suggested that the administration of quinine might counteract this tendency, and allow the remedy to be given without fear of evil consequences. Here it is, then, that the question of its usefulness first presents itself. Is there any immediate call for its employment? What good do we obtain from the combination?

The primary object in view in the alkaline treatment of rheumatic fever is not to arrest the disease, but to modify and control one very important symptom. It is, in fact, so far as I know it, to neutralise any excess of acid present in the system; and the proof that this object is gained is to be found in the presence of free alkali in the urine. So long as the reaction to test-paper is alkaline, we are justified in concluding that all the available acid is neutralised. But in practice I have found it desirable to go beyond this, and not to be satisfied unless there were a distinct deposit of phosphates. Such cases seem to me to do better than those in which the phosphates do not come down so freely; and I am disposed to urge an increase of the remedy till this result is obtained, and as soon as it has been reached to commence the quinine. If, when given in association with alkali, the urine continues phosphatic, or after the disappearance of the phosphates for a day or two their amount again becomes considerable, the dose of alkali may very properly be reduced. All who have made the experiment must have seen with surprise the complete disappearance of phosphatic deposit which occurs in so many of these cases, and must have asked themselves the question, whether the lowering effect of the alkali can depend upon the disintegration of the phosphorus compounds in the body; and if so, whether quinine can exercise any influence in arresting this decomposition of tissue?

Two cases of acute rheumatism, in neither of which the symptoms were such as to forbid some little deviation from the ordinary course of treatment, were first selected for experiment. The course pursued in each was as nearly as possible the same. After the administration of a purgative the urine passed for the following twenty-four hours was directed to be saved, its quantity measured, and a sample taken for analysis. Unfortunately in both the quantity for the first twenty-four hours could not be correctly estimated, and I thought it desirable to have a further analysis made of another case, in which this defect was remedied. Each was then ordered to take the citrate of potash draught, with a drachm of the bicarbonate added, every four hours. As soon as the urine was found to be turbid from phosphates, the urine was again saved for twenty-four hours, the whole mixed together, and a sample taken for analysis. Quinine was then given, and on two different days a further analysis of the same kind was made. Three grains of

quinine were at first prescribed, in the form of a pill, to be taken with each dose of the medicine; and as this was not sufficient to free the urine from turbidity the dose was increased to five grains, with the effect of removing it in one case, but not in the other. In the second case, a further increase to seven grains still failed in restoring transparency to the urine.

The result of these analyses gives a very definite answer to the question already propounded. The quantity of phosphoric acid eliminated from the system is not reduced by the administration of quinine; its ratio to the fluid evacuated is pretty uniform, and the actual amount secreted would seem in great measure to depend on the quantity of urine passed. So far as the alkaline treatment is a diuretic treatment, so far does it hasten disintegration of tissue generally, and the phosphorus compounds among others; but the amount of phosphates deposited has no relation to the quantity eliminated, and is simply due to alkalescence of the urine.

In the first case the turbidity was rather less, when forty-four grains of phosphoric acid were evacuated, than when only twenty-three grains were passed, because the degree of alkalescence had fallen from 0·15 to 0·04; and it had quite disappeared when the urine became neutral, though the quantity passed was upwards of thirty grains. Another important fact was ascertained in this case, viz. that the disappearance of alkalinity as a consequence of the administration of quinine is not due to the presence of uric acid, which steadily declined, both in relation to the urine passed and to the total quantity per diem, from the very commencement of treatment; its diminution being probably due to the small quantity of protein compounds introduced as food.

In the second case, we have first the negative evidence, that seven grains of quinine, taken with each dose of alkali, failed to render the urine transparent; then we have the fact, that the fourth sample of urine was more turbid than the third, corresponding with an increase of alkalescence, although the actual quantity of phosphoric acid was diminished, as well as its ratio to the water passed. The uric acid diminishes in relation to the quantity of urine, but increases in total amount at the third analysis. In the fourth experiment the uric acid was not estimated; as the alkalinity having increased, the transparent urine, after subsidence of the earthy salt, was separately examined, with a view to ascertaining the proportion of phosphoric acid precipitated to that remaining in solution. This result seemed to have no bearing on the general facts, and has not been tabulated. It served only to confirm the general truth, that the deposit bears a direct relation to the degree of alkalinity.

It has been long known to chemical pathologists that urinary

deposits, whether of lithates or phosphates, are no indication whatever of excessive elimination of either acid, but that their presence is mainly due to the reaction of the urine as to acidity or alkalescence, taken in connection with the total amount of the excretion. In this communication will be found a confirmation of this law; but it goes further, and I think I may claim for it that it establishes that the elimination of phosphoric acid is not increased by alkalies, except in so far as they act as diuretics, and that it is in no degree affected by the administration of large doses of quinine. On the other hand, when the precipitation of the earthy salts is prevented by the administration of quinine, it is due to an increase of acid, but this acid is neither lithic acid nor phosphoric acid.

It happens that when in rheumatic fever the urine is first of all turbid from the deposit of lithates, no phosphates are precipitated, the acid phosphate of lime being a very soluble salt. But as soon as a large quantity of alkali is poured into the system, this acid phosphate is converted into two neutral phosphates, of which the alkaline phosphate remains in solution, and the earthly phosphate is precipitated. This effect, however, can only be produced after all free acid in the urine has been neutralised, and a considerable excess of alkali is present; for distinct alkaline reaction to test paper may be obtained while yet there is no precipitation of earthy phosphate.

I think we may fairly draw the following conclusions: 1. That whatever other benefit may be derived from neutralising acid in acute rheumatism, its free administration gives rise to diuretic action, by which the elimination of waste material is considerably promoted, and that to secure this effect it is desirable to push the remedy till copious phosphatic deposit is obtained. 2. That the effect of quinine is not in any way injurious, and that it has no influence over the urinary secretion beyond that of occasionally rendering it less alkaline in its reaction; and that the alkali is not neutralised by any excess of either uric or phosphoric, but by some other acid.

I have not at present any further facts to bring forward to elucidate this point; but it is well known that patients suffering under nervous depression and debility are liable to have alkaline and phosphatic urine, and we, almost as a matter of course, look to quinine as one of the means of remedying such a condition. If time permit, I hope on some future occasion to try to ascertain whether this action of quinine is constant, or whether it is dependent on the previous state of the urinary secretion; and also to trace the circumstances which lead to its effects being so variable, as the evidence of these cases is quite sufficient to show. I cannot doubt the wisdom of combining its administration with the alkaline treatment of rheumatism,

though it be yet an open question how and when the combination may be employed with most benefit to the patient.

Since the foregoing observations were made, a remedy has been introduced in the treatment of acute rheumatism which bids fair to revolutionise our practice in this disease. Salicine and its compounds is not to be regarded simply as a means of relieving symptoms, but as an agent capable of subduing the rheumatism in a marvellously short space of time. I have seen a patient who, though not suffering from a very acute attack, could hardly change his position as he lay on the sofa, perfectly free from pain within twelve hours from the commencement of treatment, and walking about his room with a firm step in sixteen hours. In many cases forty-eight hours of treatment are quite sufficient to remove the pain altogether, and most commonly marked relief is obtained in twenty-four hours. I have generally selected the salicylate of ammonia, and have hitherto, in most cases administered it in half-drachm doses every two hours. In many cases unquestionably this quantity is much in excess of the requirements of the patient; but smaller doses are very often exceedingly disappointing, and many medical men who have tried the remedy have given it up, simply because of its failure in such small doses as have been frequently adopted.

I have latterly been trying to make out for myself some indication which might serve as a guide in this respect, but have not yet had sufficient experience to enable me to arrive at any very definite conclusion. Meanwhile my impression is that the wisest course to pursue is to give the remedy in full doses at first, to watch its effects, and to reduce the amount as soon as its poisonous—otherwise called physiological—action on the brain begins to be developed. I have yet to learn that if so watched it can produce harm. It is alleged that it tends to produce delirium; but among a considerable number of patients who have taken salicylate of soda or ammonia I have had only one so affected. Some of my earliest and most valuable knowledge regarding rheumatic fever was derived from a fatal case of this kind, which I had the opportunity of watching long before even the alkaline treatment was thought of, when delirium was supposed to be due to inflammation, and though bleeding had nearly passed away, leeches were still pretty freely used, to the great risk of life in such cases. In an early volume of the Reports I recorded some cases of delirium occurring under the alkaline treatment. If it should be conclusively shown that delirium was more common after the treatment by salicine than after that by alkalies, it would only be to me an additional proof of the proposition that such delirium is due to depression of the nervous centres, and is to be met by free stimulation.

With reference to the urinary secretion, one cannot fail to observe that the alkaline treatment has a diuretic power which salicine does not at all possess; and that there was comparatively little change in the characters of the urine secreted, when the action of the salicylate of ammonia was pushed to its utmost limit, and the pain had wholly subsided without any recurrence or relapse, as happened in the first case. Then, again, it is to be observed that the excretion of uric acid was in those two cases very much larger in proportion to the other constitutents of the urine than is to be found in the cases treated with alkalies. Not only so, but the actual quantity got rid of in twenty-four hours was also considerably greater. Important as these facts may be with reference to our knowledge of the action of the two substances, they do not seem to throw any light on the treatment of acute rheumatism, and afford no explanation of the rapid and powerful influence of the salicylic salt. Probably too the observations may be justly considered not quite comparable with each other, as in the last two cases the urine of three successive days was taken for analysis, while in the first three cases an interval of two or more days elapsed between the collection of the twenty-four hours' secretion. Be this as it may, I cannot but call attention to the circumstance, which seems to me deserving further study, that the acidity was so little affected either in the way of increase or diminution during the rapid cure of a disease which is so remarkable for its acid secretion. It certainly, too, throws a doubt over the conclusion which some observers have arrived at, that the symptoms are mainly due to acid present in the system, circulating in the blood, and irritating the tissues. It seems doubtful whether this acidity be more than itself a symptom of the disease, if, as in these cases, its secretion remains almost unchanged, while the patient passes from a state of extreme pain to one of perfect freedom.—*St. George's Hospital Reports, vol.* viii., 1877, *p.* 157.

24.—ARSENIC IN ALBUMINURIA.

By Dr. T. Lauder Brunton, Assistant-Physician to St. Bartholomew's Hospital; Editor of the Practitioner.

Albuminuria has been divided by Gubler into two sorts, false and true. False albuminuria, according to him. consists in the admixture of pus, or of blood, with the urine. I, however, am inclined to extend the significance of the term false albuminuria, and include in it all those cases where an albuminous body other than serum-albumin occurs in the urine. False albuminuria would thus include not only those cases where pus or blood occurs in the urine, but those in which

the so-called Bence Jones's albumin, egg-albumin, and possibly paraglobulin make their appearance. Under ordinary circumstances albuminous bodies do not diffuse through animal membranes, but it has been shown by Bernard, Pavy, Stockvis, and others that albumin of eggs will pass out through the kidneys, while the albumin of blood does not do so under similar conditions. Bernard, it is true, believed that serum-albumin passed through the kidneys in much the same way as egg-albumin when it was injected into the circulation; but Stockvis, having repeated Bernard's experiments, showed that the serum-albumin only appeared in the urine when the experiments had been imperfectly performed, and the injection had interfered with the proper circulation of the blood in the veins, thus causing venous congestion of the kidney itself. Egg-albumin and Bence Jones's albumin he found to be excreted with the kidneys, not only when they were injected into the blood, but when they were introduced under the skin, or in large quantities into the rectum. In the two latter cases the interference with the circulation was obviously not the cause of the appearance of albuminuria. It seems curious that one sort of albumen should pass through the vessels of the kidney, while another should be retained, and the only feasible explanation of it seems to be that the molecule of the different albumens varies in size. For the passage of substances in solution through membranes has been shown by Moritz Traube to resemble very much the passage of powdered matters through a sieve. When the particles of the substance are too large to pass through the meshes of the sieve they are retained, but when they are too small they pass through. Thus almost all crystalline substances readily diffuse through animal membranes, and Graham divided bodies, according to their diffusibility, into crystalloids and colloids. But there is one marked exception to the rule that crystalline bodies diffuse, and this is the colouring matter of the blood, hæmoglobin. This substance differs from most other crystalline bodies in possessing a very high molecular weight, and the molecule is therefore, in all probability, very large. Traube's hypothesis at once explains this curious exception to Graham's law, and renders it probable that hæmoglobin does not diffuse simply because its molecule is too large to pass through the pores of ordinary membranes. We may apply this hypothesis to explain the appearance of albumin in urine after the injection of white of egg under the skin, and its absence after a similar injection of serum-albumin. If we suppose that the molecule of egg-albumin is smaller than that of serum-albumin, we can readily understand that after being absorbed from the subcutaneous cellular tissue, and carried by the blood to the kidneys, it may pass through the vessels into

the urine, while the larger molecule of serum-albumin will be retained. The facts regarding false albuminuria are, that egg-albumin, and other albumins such as Bence Jones's albumin, pass out through the vessels of the kidney without any alteration in the structure of the organ, or interference with the circulation of the blood through it, while serum-albumin will not pass out.

True albuminuria consists in the passage of serum-albumin, which is a normal constituent of the blood, into the urine. It depends either upon alterations in the structure of the kidney, or interference with the circulation through it, or upon both. It has been supposed that great increase in the pressure of blood within the renal glomeruli will cause albumen to appear in the urine, but the experiments of Stockvis appear to disprove this supposition. He found that no increase in the arterial pressure, either generally throughout the body, or in the kidney alone, would produce it. He raised the general pressure by compressing the aorta and other large arteries, and he raised the pressure in the kidney itself by dividing the vasomotor nerves of the organ so that the renal arteries dilated and allowed much more blood than usual to pour into the kidney. In neither case did he find any albuminuria, but the result was very different when he interfered at all with the venous circulation of the kidney. An obstruction to the return of the blood through the renal veins was sufficient to cause albuminuria. Thus it came on when the renal veins were tied, when the vena cava was plugged, or when the movements of the heart were interfered with by a small caoutchouc ball passed down the jugular vein into the right ventricle, or when quantities of fluid were injected quickly into the jugular vein. It is to the venous obstruction caused by such injections that Stockvis attributes the albuminuria observed by Bernard after the injection of ordinary serum into the vein.

Pathologically we find albuminuria occurring from venous congestion, in cases of thrombosis of the vena cava, in mitral disease, and in emphysema whenever the right ventricle begins to yield and allow regurgitation into the veins. There is, however, another cause of venous congestion which at first sight would appear likely to have an effect exactly opposite to that which it actually produces. This is obstruction to the flow of blood through the renal artery. When this artery is ligatured, or when the circulation through the kidney is stopped by the action of drugs upon it, we find albumin appearing in the urine after the secretion has been re-established. One would expect to find the kidney pale, and anæmic after ligature of the artery, but, on performing this experiment, Brown-Séquard found that the kidney was deeply congested, the reason

of this being that the veins had lost their contractile power, and the blood had consequently flowed backwards into them from the larger venous trunks. We may, indeed, form a good conception of the condition by noticing the difference between the paleness of the hands when they are first exposed to cold and the congested blueness which follows a longer exposure. Venous congestion then is the cause of albuminuria depending on alterations in the circulation. In embolism of the kidney the result as respects a part of the organ will be the same as what we find in the whole organ after ligature of the renal artery, and the albuminuria depending upon embolism may be explained in the same way.

The second cause of true albuminuria is alteration in the structure of the kidney, and these alterations may affect the vessels and tubules or the connective tissue stroma in the which they are imbedded. In the waxy kidney the vessels are affected and the structure of their walls is changed. It seems not improbable that the altered structure of the vessels may permit the serum-albumin to transude through them in somewhat the same way as the vessels in their normal condition permit the transudation of egg-albumin. In desquamative nephritis we may suppose that the albumin finds its way into the uriniferous tubules, because the epithelial lining which might have prevented its passage has been more or less removed. Even in cases where the albuminuria depends upon organic disease of the kidney, the quantity of albumin present in the urine varies with the condition of the circulation. As a rule it is less during the night and greater during the day. We find too, generally, that a relation subsists between the quantity of urine and the proportion of albumin present in it. When the urine is copious the proportion of albumin is less; when the urine is scanty albumin is usually increased. Now it has been shown by Ludwig and Max Hermann that the quantity of urine increases with the pressure of blood in the renal glomeruli, so that, other things being equal, contraction of the arterioles just at their exit from the Malpighian capsules will increase the pressure in the glomeruli, and augment the secretion of urine. At the same time their partial contraction will probably diminish the pressure in the capillaries, will probably lessen the quantity of the lymph exuding from them into the connective tissue, and will thus lessen the leakage of albumen into the uriniferous tubules. Probably this is the reason why the albumen is usually diminished when the urine is increased, but at any rate the fact is that increased secretion does indicate increased pressure in the glomeruli, and increased secretion is usually, though not always, associated with diminution in the albumen.

I will now relate the case which I wish to bring before you, and will afterwards return to the bearing of what I have already said upon its pathology.

R.A., aged 33, analytical chemist. Short, slight, fair-haired, sallow complexion, thin. With the exception of being liable to headaches after exertion, he was apparently healthy until ten years ago. He then noticed a tendency to become very easily tired after any exertion. About a year after this (1868) he wished to insure his life, but was rejected on account of the medical officer of the insurance company having discovered albumen in his urine. He then consulted two medical men, who gave him strychnine in doses of about $\frac{1}{30}$ of a grain. This immediately stopped the albumen, but brought on violent headaches and sickness. The albumen at this early stage was only present during the summer months. It came on with work and disappeared with rest. In the winter it was absent, except during one or two severe frosts.

In the following summer it returned in large quantities, and he was advised by his medical man to go to the seaside (Margate and Ramsgate) for the three hot months, to take hot sea-baths, and to abstain from all exertion.

Though the albumen ceased, the patient's strength diminished very much. On his return to London the weather was cooler and he rapidly improved, so as to be able to return to work. In 1870 the albumen returned more persistently, so he was again recommended by a medical man to take a sea-voyage. He went to the Cape of Good Hope; leaving in June and arriving in November, very little, if at all, the better for the trip. This might be due to his ignorance of the care required in selecting the proper food. He stayed six weeks at the Cape, then came back. On his return he tried pancreatic emulsion, and for two or three days it was very successful and stopped the albumen, but after that time it brought it on worse than ever. He then tried the skim-milk treatment for three months. It was very successful at first, but he lost weight rapidly, and the albumen gradually reappeared during the last two weeks of the treatment. On discontinuing it the albumen returned.

In 1872 he was again recommended to take another and longer voyage, and take care of his diet on the way and after his arrival. He had now found that fatty food always brought on albumen, and that meat taken in the morning had a similar effect, but he could take meat with impunity for supper. He now went to Australia and New Zealand for nearly three years, leaving in October 1872, and returning in June 1875. During the greater part of this time phosphates were very abundant, and although albumen was generally completely absent, it came on whenever he exerted himself much. His

strength also did not increase much. On one occasion he took a prescription containing quinine and phosphoric acid, which almost immediately caused albumen to appear in considerable quantities, and was some time before it again disappeared. After his return in June 1875, he still remained in *statu quo*.

I saw him for the first time in January 1876. No œdema; the heart and lungs were healthy; liver normal; appetite poor; subject to acidity and headache. On inquiry I found that the albuminuria was brought on by exertion either mental or bodily in the morning, by fats such as butter, or by meat for breakfast. After midday he could do work, and could take fat and meat without bringing on the albumen. The quantity of albumen was always greatest when the urine was scanty, and least when the urine was abundant. When he engaged in his work during the forenoon albumen did not come on immediately after breakfast, but began to appear about eleven and remained until about two. When he did not work the albumen did not come on at all. He suffered much from acidity, especially in the morning. The remarkable fact that meat and fats caused albumen to appear, and that by rigid adherence to a farinaceous diet he could completely get rid of the albumen, led me to think that his albuminuria might be connected with imperfect digestion, and I accordingly gave him nitro-muriatic acid before meals. This lessened the acidity, and diminished, but did not completely remove the albumen, so that I supposed the kidneys to be also in fault.

The quantity of albumen, as I have already mentioned, was always greatest when the urine was scanty, and least when the urine was abundant. This seemed to indicate to me that the albumen came either from the venous radicals in the kidney, or else from the lymphatic spaces in the connective tissue. I therefore attempted to act upon the arterioles of the glomeruli, and increase the secretion of urine by means of digitalis. The experiments made by Mr. Power and myself had shown us that this drug has a special action upon the vessels of the kidney, and, by giving it in small doses, I expected to contract the arterioles of the glomeruli, and thus while increasing the urine to diminish the pressure in the capillaries and veins, and lessen or remove the albumen. I intended, in fact, to produce by the drug the condition in which the patient had already found there was less albumen.

Two drachms of the infusion three times a day diminished the albumen, but lowered the action of the heart and interfered with indigestion. After reducing the dose and finding that still the digitalis disagreed, I stopped it after it had been taken for a fortnight.

There was no history of sypbilis, but thinking that the alter-

ative action of mercury might be beneficial, I tried small doses of hydrarg. c. creta, but without any beneficial result. I next tried quinine and sulphuric acid, but this brought the albumen in large quantities into the urine, doubled it in fact, within twenty-four hours, just as it had done before.

As he had found benefit from pancreatic emulsion for a day or two on a former occasion, I advised him to try it again, and to stop it after two days so as to prevent the increase of albumen which its continued use had previously induced. The first dose, however, made him worse, and it was accordingly stopped. My attempts to act upon the vessels of the kidney having been futile, and my random treatment with mercury and quinine having also been useless, I determined to try to act upon the secreting structures of the kidney, and accordingly gave arsenic, which has a considerable action upon tissue change, and appears to possess also a special affinity for epithelial structures.

The patient accordingly took 3 ♏ of Fowler's solution at meal-times. Almost at once the albumen disappeared, and the patient was able to do much more work than usual without bringing it back. After giving this for a while I thought that as phosphorus is nearly allied to arsenic the hypophosphites might have a similar action, and accordingly gave him 5 grs. of hypophosphite of soda three times a day. This disagreed with his stomach and increased the albumen. He then returned to Fowler's solution again, and the albumen disappeared. On stopping the medicine the albumen came back, but the liquor sodæ arseniatis stopped it.

On again thinking over the pathology of the case it seemed to me probable that the albumen was derived in great measure, from the intestinal canal, and was due to imperfect digestion of albuminous substances which were absorbed from the intestine, and excreted in the urine in much the same way as white of egg would have been if the person had swallowed several raw eggs at once. As the pancreatic juice first converts coagulated albumen into a soluble form before finally digesting it, it seemed probable that imperfect pancreatic digestion was the cause of the albuminuria. The failure of the pancreatic emulsion to do good might have been due to the fat causing acidity of the stomach. The chyme being too acid would prevent the pancreatic juice from acting, and would thus make matters worse. I therefore gave pancreatine without the fat, so as to increase the pancreatic digestion without increasing the acidity of the stomach, and this has stopped the albumen also. He has, however, only been taking this for a short time, so that I do not know what the result will ultimately be.

I am inclined to regard the case I have described as due at

least in part to imperfect digestion. I am uncertain whether the kidneys have anything to do with it or not. There is no history of nephritis, and the close connection between the digestion which is weak in the forenoon and stronger in the afternoon, the acidity of stomach, and albumen in the forenoon, and the want of these in the evening, seem to point to indigestion as the chief cause of the albuminuria. I have not seen any casts, but I have not examined the urine frequently for them. The albuminous body which appears in the urine is probably not serum-albumin, because it only coagulates between 175° and 180° F. while the albumin in a case of chronic Bright's disease which I tested for the purpose of comparison coagulated at 140° F. The point of coagulation varies somewhat according to the amount of urea and uric acid present in the urine, but this variation will hardly account for the difference of more than 30° F. between the coagulating points of the albumen in these two urines.

The points which I wish especially to bring forward are:—1. The intermittence of the albumen and its connection with the digestion; 2. The absorption of albuminous matters from the intestine as a cause of albuminuria—a cause recognised indeed in several text-books, but often ignored in practice; 3. The utility of arsenic as shown in this case; 4. The possible use of pancreatine; 5. The effect of quinine in increasing the albumen.—*Practitioner, June* 1877, *p.* 427.

25.—ON THE PATHOLOGY OF URÆMIA AND THE SO-CALLED URÆMIC CONVULSIONS.

By Dr. F. A. Mahomed, Pathologist to St. Mary's Hospital, and Assistant Physician to the London Fever Hospital.

That Bright's disease is the most frequent cause of cerebral hemorrhage is, perhaps, one of the most universally admitted pathological facts; but the statement is usually made with regard to the grosser hemorrhages, which cause death from apoplexy. It is also generally admitted that small capillary hemorrhages frequently accompany and are seen on the outskirts of the grosser lesions; but that such minute hemorrhages occur alone, and without association with the larger ones, though under the same conditions, does not appear very generally recognised, and certainly the symptoms to which they give rise are altogether overlooked. All pathologists mention such conditions as occurring, usually in connection with disease of the vessels and other conditions to be considered hereafter, but very few attempt to associate them with any symptoms.

Wilks and Moxon, speaking of them, say; "This condition of the brain denotes a diseased state of the blood or the vesssel

generally, and is found in purpura, in idiopathic anæmia, and sometimes in fever. We think it interesting in connection with paralysis which may come on in cases of fever shortly before death; we have then found this state of capillary apoplexy to exist. It is often present also after the *convulsive attacks* occasionally observed in typhus."

Dr. Hughlings Jackson, in his admirable article on Apoplexy, in Reynolds' System of Medicine, comes nearer the truth. While discussing the frequent association of Bright's disease with apoplexy, he mentions capillary hemorrhages, which, he states, are sometimes found, and he thinks often occur in this disease; concerning them, he speaks thus: "We have, unfortunately, very little precise information about the symptoms they produce, as they do not cause death."

Rindfleisch describes these capillary hemorrhages more accurately than any other pathologist, and altogether places them in a more important position, though chiefly using them as types of cerebral hemorrhage, which are serviceable for microscopic demonstration of the minute changes produced by hemorrhage into the brain-tissue. He mentions their association with grosser hemorrhages in Bright's disease; and indicates that, in this condition, hemorrhages various in extent may occur and clots of various sizes result. He, moreover, divides them into two classes: punctiform hemorrhages, the results of "diapedesis" or simple exudation, as in purpura, &c.; and those the result of absolute rupture of walls of vessels, such as occur in Bright's disease, the result of degeneration of the walls of the vessel by endoarteritis or periarteritis, and of the increased tension within them. He also points out that the extent of the hemorrhage depends upon this amount of arterial tension.

Niemeyer suggests that these capillary hemorrhages give rise to the so-called "warnings" preceding an attack of apoplexy.

To form an opinion, first, as to the question what evidence there is that these punctiform hemorrhages may give rise to convulsions such as we meet with in so-called uræmia, let us examine the pathological conditions under which it occurs and the symptoms associated with them. On examining that admirably arranged storehouse of facts the *post mortem* records at Guy's Hospital, limiting my inquiries to the last six years, the conditions in which punctiform hemorrhages have been found in the brain may be tabulated with some from other sources as follows.

I. Hemorrhages from simple exudation, due to altered blood; hemorrhages by "diapedesis."

1. Purpura.
2. Leukæmia.
3. Malaria.

II. From bruising, due to laceration of vessels: hemorrhages by "ecchymosis."

1. Injuries to skull and lacerations of brain, from blows, &c.

III. From inflammations or congestions.

1. Cerebritis, including also
   a. Red softening;
   b Invasion of cortex by meningitis.
2. Embolism.
3. Fevers.
4. Sunstroke.
5. Occasionally the vicinity of tumours, aneurisms, hydatids, &c.

IV. From diseases of vessels, permitting rupture by blood-pressure.

1. The psychoses (Rindfleisch.) General paralysis.
2. Bright's disease.
   a. In the neighbourhood of larger clots.
   b. Alone, in deaths from uræmia in which convulsions have been present during life.

In considering this table especially in relation to the question of convulsions accompanying the diseases mentioned, the results are, I think, most striking and important. In the first class, in which blood exudes passively and without any increase of pressure upon or laceration of the cerebral substance, convulsions are not present, or, if ever, very rarely. In the second class, due to injuries, all symptoms are masked by those of concussion and paralysis. Moreover, in them the pressure on the cerebral tissue is not increased. In the last two classes, which are characterised by hemorrhages *with increased blood-pressure*, convulsions are most common, in many of them almost invariable; and, inasmuch as hemorrhages do not occur in *all* these cases, it would not be too much to say, I think, as there is some evidence to support it, that those cases in which convulsions occur are those in which these capillary hemorrhages are present, though not to such a degree as to cause absolute paralysis, which it appears equally certain that they may do if very numerous.

But it may be said, Why, if these hemorrhages be the cause of uræmic convulsions, are they not more frequently found in the deadhouse associated with Bright's disease? I think there are sufficient reasons to answer this question satisfactorily. First, such small hemorrhages as these would be rapidly absorbed; and, if the convulsions had not occurred within a few days of death, they would not be found. Secondly, cases dying in this manner, though not unusual, are not so very common. Thirdly, the head is sometimes not examined at all; and, if

examined, it may be done without very great care. Fourthly, it is impossible to examine the grey matter of each convolution minutely, so that such small hemorrhages might easily be overlooked even by a most careful pathologist. Now, however, thanks to our ability to localise the disease more accurately, we may hope to discover them with greater ease.

It is of interest also to note, as bearing upon the question of these hemorrhages, that uræmic convulsions are more common in acute Bright's disease; while larger hemorrhages, causing death from apoplexy, occur more frequently in the chronic form and in more advanced life. The reason for this is very evident. It is not difficult to rupture the capillaries by increased blood-tension, owing to the thinness of their walls, as seen in the kidney when bloody urine is produced, this being undoubtedly due to rupture of capillaries; while in the chronic disease the larger vessels have had time to undergo degeneration, so that they become equally, if not more, liable to rupture.

Another question may be put, and one which bears strongly upon this subject, and indeed forms part of it. Supposing these hemorrhages are the cause of uræmic convulsions, how do you account for the other symptoms of uræmia? To this I would answer: By œdema of the brain-tissue. I know that the occurrence of such a thing as œdema of the brain has been denied by many eminent pathologists; but, on the other hand, it has been supported by others. Of late years, I think, many new facts have been obtained in relation to this question and very strongly supporting such a view. The most important of these is the hyaline exudation found by Sir William Gull and Dr. Sutton around capillaries in nervous tissues: this they have found, if I remember rightly, in acute Bright's disease associated with dropsy elsewhere. I know that it was under conditions of increased vascular tension. This hyaline exudation is, moreover, also described by Rindfleisch, and by Cornil and Ranvier. It is of the same nature as œdema of the brain, only the exudation has been highly fibrinous and has coagulated, thus forming the only condition under which it could be examined microscopically. Another fact of a somewhat similar nature is the extreme hyperæmia, accompanied by transudation of leucocytes through the walls of the vessels, found in hydrophobia by Dr. Coats, Dr. Gowers, and others. Moreover, accepting the mechanical theory of the production of dropsy, which now-a-days must surely be almost universally admitted, I can see no reason why the greatly-increased blood-pressure in Bright's disease should not cause transudation of serum through the walls of the cerebral capillaries, which are amongst the most delicate in the body, as through those of

other parts. Of course, owing to the density of the structure surrounding them, and the intracranial pressure, we should expect it to be the last place in which it occurred; but so indeed it usually is, as evidenced by the symptoms produced. But we also know that there are times at which the pressure in the blood-vessels in the brain exceeds the pressure outside them, as must necessarily be the case in all hemorrhages; when this occurs in the capillaries, what more probable than exudation of serum or rupture and punctiform hemorrhages, according to the degree of intensity of blood-pressure? If, then, œdema be admitted as possible, the other symptoms of uræmia—the coma, drowsiness, and typhoid state—are completely accounted for by compression of the brain-tissue.

It may be objected to this, that uræmia occurs in chronic Bright's disease in which there is no dropsy. I have before pointed out a probable reason for the absence of dropsy in this condition; namely, the insidious nature of the disease, the gradual increase of the blood-tension, and the accompanying gradual thickening of the capillaries, which are thus enabled to withstand the increased blood-pressure. But, when this is still further suddenly increased, as by the supervention of an acute attack on the chronic disease, dropsy does occur, and so does uræmia; but apart from this, the occurrence of a great increase of tension, causing distension of capillaries without exudation, would, by increased pressure on the nervous structures, produce some of the slighter symptoms of uræmia, such as the characteristic headache of Bright's disease.

To return, however, to the convulsions, other grave objections may be raised to my theory of their production.

1. Experimental evidence may be adduced to show that the grey matter of the cortex may be cut, bruised, burnt by caustics, and irritated by various methods, without the production of convulsions. But, on the other hand, besides its irritation by electricity, Nothnagel produced convulsions by the injection of a solution of chromic acid into the grey matter of the cortex. But, more than that—for experimental evidence, unless supported by pathological facts, is worth but little—I think I have already adduced sufficiently strong evidence of the association between punctiform hemorrhages and convulsions to be found in the deadhouse.

2. It may be said that the convulsions produced the hemorrhage, and not the hemorrhage the convulsions; but the entire weight of pathological evidence is the other way. People dying of epilepsy do not usually present these hemorrhages; in fact, all authorities on this disease emphasise greatly the fact of their great rarity. While in the deadhouse it is common to see ecchymoses below the skin, serous and mucous membranes, the

result of impeded venous circulation, as in death by asphyxia from any cause, it is extremely rare to see these hemorrhages in the brain-tissue under these conditions; though they will occur beneath the arachnoid, I have never seen them, nor can I find any record of their occurrence, in the substance of the brain, from this cause.

3. Epilepsy may be quoted as a fact against my theory, for not only are these hemorrhages not found in epileptics, but epilepsy is believed by many eminent authorities to be associated with anæmia of the brain rather than hyperæmia; but this view is at least open to question. It would be impossible to discuss this subject here; but however strong the evidence of anæmia of the brain in epilepsy may be, the side of hyperæmia is almost equally strong. If Kussmaul and Tenner think they have proved *experimentally* that anæmia of the brain produces convulsions, so also Schroeder van der Kolk has beyond doubt, I think, proved that dilatation of the capillaries occurs as the result of hyperæmia in epileptics; and this, be it remembered, not generally throughout the brain, which might be the result of general turgescence owing to impeded venous return, but in that centre in the medulla to which he refers the irritation producing the convulsions. He is even able to divide his cases into two classes: tongue-biters, in which the capillaries of the hypoglossal nucleus are especially affected; and those who do not bite the tongue, in which those of the pneumogastric nucleus are alone dilated.

While suggesting capillary hemorrhage as a cause for convulsions, let it be clearly understood that my remarks are confined to those occurring in conditions of high arterial tension, as in Bright's disease, pregnancy, and other conditions in which so-called uræmia occurs. The convulsions of epilepsy are probably due to some hyperæsthetic or irritable condition of nerve-cells which may be due to a local hyperæmia, or to some other cause, but certainly not to capillary hemorrhage.

4. Dr. Hughlings Jackson's theory of spasm of cerebral arteries as a cause for uræmic convulsions remains to be contested: it must stand or fall in great measure with the anæmic theory of epilepsy; but, even admitting that to be true, it appears to me difficult to conceive such severe spasm of cerebral arteries as to so completely cut off all blood-supply to the brain, as Kussmaul and Tenner's experiments seem to indicate as necessary for the production of epilepsy; for, according to them, if but one vessel out of the four large arteries supplying the brain be left uncompressed, the convulsions did not ensue.

5. Dr. George Johnson's theory of "stop-cock action" of arterioles, and Dr. Gower's recent ophthalmoscopic observations in support of this, may also perhaps be adduced as reasons

against the hemorrhagic theory of convulsions. To take the latter observations first, I think they are open to error. Any one who has used the sphygmograph extensively, will recognise the fact that arterial tension may be high—that is, the tidal wave sustained—even with a relaxed condition of at least the larger vessels, while in other cases the arterial system throughout is contracted; every degree, from great relaxation to tight cord-like contraction of the vessels, may accompany increased arterial tension. In all alike, cerebral hemorrhages are liable to occur. I think Dr. Gowers' observations can only show this contracted state of the vessels generally or their relaxation, but I do not think that they are any test of arterial tension.

If Dr. Johnson's view of the stopcock action of the arterioles be true, then the tension in the capillaries must surely be low, or not increased, owing to the defending contraction of the arterioles; and it is this fact chiefly that makes me unable to accept his otherwise ingenious theory, for, without admitting that the high tension extends to the capillaries, I cannot see how to explain bloody urine and dropsy in acute Bright's disease, and the hyaline thickening around capillaries and absence of dropsy in chronic Bright's disease.

These, then, are the reasons for and against this theory of the *vascular cause* of what are known as uræmic symptoms, as far as they occur to me. I think they would be better described as the symptoms of high tension in the cerebral capillaries, or, more shortly, as those of *cerebral œdema* or hemorrhage. They must always be regarded as a precursor of apoplexy, if they occur in a person advanced in life or with degenerate vessels. Should my view prove to be the correct one, the indications it affords for treatment by rapidly diminishing the blood-pressure are of the utmost importance for the safety of the patient.—*British Medical Journal, July* 14, 1877, *p.* 42.

26.—NEW URINARY TEST-CASE.

By Dr. Rayner W. Batten, Gloucester.

Every medical man must at times have felt the inconvenience arising from not having the means with which to examine the urine of a patient whilst still at the bedside. The ordinary urine-cases are too large to be always carried about, whilst there is with them generally also the risk that their contents will be spilled.

I have been asked to bring under the notice of the profession a small case which can easily and safely be carried in the waistcoat-pocket, and which for the last two years I have found to answer its purpose extremely well. It consists of an aluminium case about four inches long and half an inch in diameter, re-

sembling in appearance the ordinary pocket caustic-holder, and similarly divided into two compartments. The smaller compartment has within it three vulcanite specific gravity beads, marking the extreme and mean specific gravity of ordinary urine. The larger compartment contains a test-tube, within which are three or four capillary-tubes, charged with nitric acid, and a bottle for Fehling's test-solution; this bottle is closed with an India-rubber stopper, upon which the alkali has no action, and hence the copper solution will keep good and clear it as long as may be required. The screw junction uniting the two parts of the case is itself hollow, and holds, excluded from the air, the red and blue litmus paper. A wineglass, and a candle or lamp, which can always be obtained, are all that can be further required. It is, perhaps, scarcely necessary to add that, by holding the test-tube in the blue portion of a flame, all smoking of the glass is prevented. The specific gravity beads will be found to tell the specific gravity of the urine very accurately; whilst they have the advantage, a most important one at times, over the ordinary urinometer, that a very small quantity of urine, two or three teaspoonfuls even, will be sufficient for the purpose.

To those who are unaccustomed to the use of the nitric acid tubes, I may say that the best mode of using them is first to shake the acid down to one end of the tube, then to break off the other end by a sharp rap of a knife or other hard body; on inserting the open end of the capillary-tube just within the mouth of the test-tube, and heating the other end of the capillary-tube in the flame of the lamp, all the acid will be driven into the test-tube.

This pocket urine-case has been very neatly and admirably made for me, after a considerable amount of trouble on their part, by Messrs. Salt and Son, the surgical instrument makers of Bull Street, Birmingham. All its parts are made to measure, so that, at any time, the bottle, test-tube, or charged capillary-tubes, of the proper size and length can be obtained from the makers by return of post.—*British Medical Journal, Oct.* 13, 1877, *p.* 528.

SURGERY.

FRACTURES, DISLOCATIONS, AND DISEASES OF THE BONES AND JOINTS, ETC.

27.—EXPERIMENTS ON FERMENTATION.

By Joseph Lister, Esq., M.B., F.R.S., Professor of Clinical Surgery and Surgeon to King's College Hospital.

In medicine, the large class of diseases termed zymotic derive their name from the hypothesis that their essential nature is fermentative. In obstetrics, puerperal fever, the most frequent cause of disaster after childbirth, is now regarded by many of the highest authorities as likewise due to fermentative disorder; and, in surgery, among the various causes which may disturb a wound, we know that by far the most frequent in operation, and the most pernicious agent in its effects both upon the wounded part and upon the constitution, is putrefactive fermentation. If this be so, it is clear that to understand the nature of fermentation must be a matter of the very highest importance, with a view to curing or preventing the various evils to which I have alluded.

What, then, do we mean by fermentation? I shall best approach the answer to this question by giving an example. Rather more than a week ago, I witnessed in the north of Italy the time-honoured practice of treading grapes in the wine-vat. I was told that the juice would within twenty-four hours boil, as it was said, over the vats into which it was introduced; in other words, that the sugar of the grape-juice would within that short time be so converted into alcohol and carbonic acid gas, that the carbonic acid gas, by its ebullition, would cause sufficient frothing to produce the effect to which I have referred. This conversion of the sugar of the grape into alcohol and carbonic acid gas is accompanied by the development of a microscopic organism, the yeast-plant, or, to continue the old nomenclature, *Torula cerevisiæ*, consisting of microscopic cells multiplying by pullulation, as indicated in this rough diagram. Now, it is, I believe, universally admitted that the alcoholic fermentation of grape-sugar is due to the growth of the yeast-plant. M. Pasteur thinks that he has traced the origin of the yeast-plant in the juice of the grape to a minute fungus adhering to the outside of the skin of the grape. Be this as it

may, it is admitted on all hands that the alcoholic fermentation is caused by the growth of the yeast-plant. So long as the juice of the grape is protected by the skin of the berry, no fermentation occurs; but as soon as it escapes from that protection, the organism, by its development, induces the fermentation. Nor is it by any means exclusively in the natural juices of fruits that such fermentation is induced. Any sugary solution, provided it contains, besides the sugar, other ingredients requisite for the nutrition of the yeast-plant, will serve as pabulum for the yeast-plant, and in this case the yeast-plant will give rise to the fermentation. Here is a glass containing what is termed Pasteur's solution, a solution devised by M. Pasteur for the very purpose of affording nourishment to the yeast-plant and other minute organisms. This was prepared on September 10th in a flask purified by heat, covered over with a pure cotton cap, which permits the entrance of air, but does not permit the entrance either of the yeast-plant or of any other form of dust. The Pasteur's solution, of itself containing, besides sugar, ammoniacal and earthy salts for the nutrition of the fungus, was heated to about the temperature of boiling water, so as to destroy any organisms that might exist in the water. The result is, that it continues perfectly unchanged, just as it was on September 10th; but, if we were to add to it a little of the yeast-plant from fermenting grape-juice, we should find that, at the temperature of summer weather, this would very soon be in a state of free fermentation at the same time that the yeast-plant would multiply. This, then, is a typical instance of fermentation. We have an active agent termed the ferment, which ferment is capable of self-multiplication. That I believe to be the essential property of a true fermentation. There is an active principle termed the ferment, which ferment has the faculty of self-multiplication. Now, in this particular case, I have already said it is admitted on all hands that the yeast-plant is the cause of fermentation. Persons may differ as to how the development of the yeast-plant gives rise to the resolution of the sugar into the alcohol and carbonic acid gas; but all now agree that, somehow or other, the yeast-plant causes the fermentation. Now, is this so in all cases, in the case of all fermentations? Are all true fermentations caused by the development of organisms? That is the question which it is desirable that we should be able to answer.

Take, for example, the case of the putrefactive fermentation of blood. We all know that, if blood be shed from the body into any vessel without special precautions, in a few days it putrefies. The bland nutrient liquid, soon after leaving its natural receptacle, become foul, acrid, and poisonous: a change fully as striking as the change which sugar undergoes in the

alcoholic fermentation. Here we have a glass into which blood was received with special precautions. In the first place, the glass, covered, as you see, with a glass cap and a glass shade, with a view of preventing the access of dust, and standing upon a piece of plate-glass, had been heated to about the temperature of 300 deg. Fahr., and cooled with an arrangement that insured the filtering of the air from its dust—the air that entered during cooling—so that we were perfectly sure that the glass contained no living organisms.

Then, in the second place, the glass had been charged from a flask like this. It contains, as you see, a glass tube introduced into it; it is stuffed well with cotton-wool between the neck of the flask and the tube, there is a piece of cotton-wool over the end of the tube, and another piece is tied securely over the spout of the flask. The flask so arranged was heated just as the glass had been heated. It is not necessary to heat so high as to singe the cotton. Heat far short of this is adequate, according to my experience, to make perfectly sure that you destroy all living organisms. The flask having been thus prepared, the jugular vein of an ox was exposed, with precautions against the entrance of anything putrefactive, and, the cotton cap having been taken off from the end of the tube, the jugular vein was slipped over the tube, tied on, and then the hand of the assistant, who previously restrained the flow of blood, being relaxed, blood was permitted to flow into the flask. Then, before coagulation had time to take place, this and various other glasses were charged after the removal of the cotton cup from the end of the spout. Now, the first thing that may strike you is the remarkable fact that this blood-clot has not undergone any contraction. One of the earliest things that your professor of physiology will have to teach the junior students will be that blood, after coagulation, contracts; that the blood-clot contracts; that the fibrin is pressed out. But here no such thing has taken place. There has been no shrinking of this clot, no pressing out of the fibrin, and I venture to say that there is no one here, at least I think it is unlikely that there is anyone here except myself, who has seen such a phenomenon, illustrating how, when the most familiar objects are placed under new circumstances, the most unexpected results will arise. Now, this is a matter of very considerable interest with reference to the behaviour of blood-clots inside the body in wounds and so forth. However, that is not the point to which I wish now to draw your attention. The point to which I wish to draw your special attention is, that this blood, although it has been six weeks in this glass, without any close fitting of the glass shade or the glass cap, has not putrefied. The air is perfectly sweet, perfectly free from odour.

Now this, without going further, is a very important matter. It proves that the blood has no inherent tendency to putrefaction. It further proves that the oxygen of the air is not able to cause the blood to putrefy, as used to be supposed. There was a time—the effect is still seen to a certain extent—when the dark venous colour of this blood-clot gave place to the crimson colour of arterial blood in a gradually deepening band from above downwards. We still see some of the red colour remaining, though now the converse effect has begun to take place. That florid redness, then, showed that the oxygen of the air was in reality acting upon the blood, yet it did not putrefy. Now, if I were to take a little morsel of already putrefied blood, say, upon the end of a needle, and touch with it this clot of blood, putrefaction would, in the course of a very short time, spread throughout the mass. Exactly as in the case of alcoholic fermentation under the influence of a yeast-plant would the fermentation spread.

Putrefaction, then, is a fermentation, a true fermentation, characterised by the power of self-propagation of the ferment. Then, if we examine microscopically, we find in the putrefying blood, as we found in the fermented grape-juice, microscopic organisms, termed bacteria from their rod-shape, which we have here represented on the same scale as we had the yeast-plant; some of different sizes, very much more minute than the yeast-plant, and commonly endowed with a remarkable power of locomotion. I say that, in the putrefying blood, we find these organisms developing *pari passu* with the fermentation.

Now, the question is, Are these bacteria the cause of the putrefactive fermentation, or are they merely accidental concomitants? These are two views which are entertained at the present day by men of high eminence. It may be said, "Why should there be any doubt that the bacteria are the cause of the putrefactive fermentation, any more than there is a doubt that the *Torula cerevisiæ* is the cause of the alcoholic?" Well, one reason I believe to be that the bacteria are so exceedingly small. They are not so easily defined. We cannot get them in a mass as we can get a mass of yeast; at least without a great deal of trouble; and, besides that, they occur very similar in appearance in a great number of different fermentations. There is, therefore, so far some colour for doubting whether bacteria are the cause of a special fermentation, like this putrefaction. Then there is another ground justifying such a view; for certain it is that organic substances are liable to extremely remarkable alterations, decompositions, under the influence of agents which are endowed with no life at all. As good an example of this as we can take is what occurs in the bitter almond when it is bruised with water. You all know what takes place under

those circumstances; that there is prussic acid developed, and essential oil of almonds, and other materials. Now, these did not exist beforehand in the bitter almond, but they are the result of the mutual action upon each other of two constituents of the bitter almond, neither of which was hydrocyanic acid, nor oil of bitter almonds, &c. These two constitutents are termed emulsin and amygdalin. Amygdalin is a crystallisable substance, and can be obtained separate. Emulsin, though not obtained in a state of crystallisation, can be obtained separately. Till these two materials are in a state of solution in water, they do not act upon each other, upon the bitter almond at all; but, as soon as they are in watery solution, the emulsin so acts upon the amygdalin that the amygdalin becomes broken up into the constituents to which I have referred. This is an exceedingly remarkable fact. Undoubtedly, the emulsin is dead; there is nothing living about it. It is not an organism. It is obtained by a process of alcoholic extraction and so forth. It is thoroughly a chemical substance, a merely dead substance, if we may so speak, and yet it does produce this remarkable effect upon the amygdalin. But, when we come to consider this case, we find that this process, remarkable as it is, lacks the true character of genuine fermentation, that of the faculty of self-propagation of the ferment. Liebig himself, who was the great advocate of the doctrine of so-called chemical ferment, and who discovered this action of emulsin on amygdalin, pointed out, and showed by irrefragable evidence, that the emulsin does not undergo any multiplication; not only so, but that, after a while, it so happens that the emulsin loses the property of acting on the amygdalin; but, for a considerable time, it continues to act upon it without undergoing apparently either increase or diminution of its bulk. It may be called a resolvent, the amygdalin being the resolved material.

There are other cases equally striking that might be mentioned, not only in the chemistry of vegetables, but in the chemistry of our own bodies. There exists, for instance, in the saliva a material called ptyalin, which has a remarkable power of acting upon starch. In the gastric juice, there is a material called pepsin, which has a remarkable power undoubtedly of acting on albuminous materials, fitting them for solution in digestion. But here again we find, when we come to consider the matter, that there is no evidence whatever that either pepsin or ptyalin is capable of self-multiplication. Each is secreted for the purpose and in the quantity in which it is required, but it has no faculty of self-multiplication; and I believe, if you search through the whole range of organic chemistry, you will not find a single recorded instance where any ferment, so-called, destitute of life has been proved to have the power of self-

multiplication. At the same time, though this is the fact, I believe I am correct in stating that it may be admitted that the thing might be theoretically possible. It is conceivable, for instance, that a resolvent, if we may so speak, of comparatively simple constitution might, by its action upon a resolvable compound, resolve it into substances, one of which should itself be the resolvent, and, if that were so, the process might go on *ad infinitum.* That is conceivable; and even, if it were not conceivable, although we have no instance of the kind on record, yet we have persons in high authority, as teachers both of physiology and of pathology, maintaining this view: that probably, under the circumstance, for instance, of putrefactive fermentation, the bacteria are mere accidental concomitants; but the real essential agent in the putrefaction is not an organism at all, but some so-called chemical ferment destitute of life. I say, so long as we have authorities maintaining such a view, it is our duty, if we can, to disprove it; and it has been with this object that the investigations of the last two months, to which I have referred, have been conducted.

I may say that, as regards the putrefactive fermentation, we have already evidence in the flask and in the glass that I have shown you (the flask also has no putrefactive odour emanating from it), that blood has in itself no inherent tendency to putrefy. It must receive something from without, and that something is not mere oxygen. Mere oxygen will not do; mere water will not do. Blood and water constitute a mixture highly putrescible, very much more liable to putrefaction than blood itself. But here we have had mixed with water the contents of a glass like this, only that the water had been previously boiled, so as to kill any organisms in it; boiled under the protection of a cotton cap, and then, the cotton cap being raised, careful provisions (into which I must not enter) against the entrance of dust being taken, the clot was spooned into the water; a fresh cotton cap, perfectly pure, was put on, and so we got, I believe for the first time, a permanent cold watery extract of blood, and here it retains the same brilliant clearness that it had in the first instance, more than a month ago.

The special process of fermentation which I have been investigating has not been the putrefactive, but one which seemed to me more convenient for the purpose, the lactic fermentation, the fermentation by means of which milk sours and curdles, by means of which the sugar of milk, instead of being converted as grape-sugar into alcohol and carbonic acid, is converted into lactic acid: a curious instance of a chemical alteration. The chemical composition, as regards the proportions of the three elements, carbon, hydrogen, and oxygen, remains identically the same; but those of you who are

chemists understand what I mean when I say the atomic weight of the lactic acid is one-fourth of the atomic weight of the sugar of milk. Each atom has been resolved into four simple atoms of lactic acid. Now, it may be naturally supposed, if you observe what happens in a portion of milk obtained from a dairy, that this is an absolute inherent tendency of the milk, this souring and curdling. If you get milk from a dairy and keep it long enough, it is certain to turn sour and curdle; then after a while, there comes a certain mould upon the surface, the oïdium lactis, which constitutes the sort of bloom there is upon a cream cheese; then afterwards comes on, often simultaneously with the growth of this mould, the butyric fermentation, in which butyric acid is produced; and afterwards, if you keep the milk long enough, it will probably putrefy. When you see, time after time, specimens of milk, taken from various dairies, undergo this succession of alterations, you may be tempted to suppose that these were changes to which the milk was disposed from its own inherent properties as it comes from the cow's udder. The late eminent Professor of Chemistry in this College, Professor Miller in his excellent work on Chemistry, states that the ferment of the lactic acid fermentation is the caseine of the milk. I am bound to say, however, in justice to Professor Miller, that he also adds that M. Pasteur has expressed his belief that there exists an organic living ferment which produces this fermentation; but Professor Miller does not profess to decide between these two opinions. On the contrary, his first statement, that the caseine is the ferment, might lead you to suppose that he inclines to the former view. If this were the case, as there is caseine always in the milk, there should always be the lactic acid fermentation. But it was pointed out long ago by M. Pasteur that, if you examine any specimens of souring milk with the microscope, you find little organisms. These, when you come to look at them carefully, you see to be obviously of the nature of bacteria. Bacteria may either have the faculty of motion or they may not. This particular bacterium is a motionless bacterium, so far as I know; still it has the essential nature of a bacterium: a microscopic fungus, multiplied by fissiparous generation, always by lines transverse to the longitudinal axis of the organism. I have ventured to give to this little organism the term bacterium lactis; for, no doubt there are different kinds of bacteria. The mere fact that they are minute must not make us shut our eyes to this fact. You sometimes hear bacteria spoken of as if they were all alike. The fact that some do not move and others do, is one indication of a difference. Another indication of a difference is, that some bacteria will thrive in a substance in which others cannot

live. For instance, the bacterium lactis refuses to live at all, according to the more careful experiments I have been lately making, in Pasteur's solution; the very fluid provided by Pasteur for bacteria to live in, and for these torulæ and for fungi generally, that is a medium in which the bacterium lactis refuses to grow at all, although the majority of bacteria grow in it with rapidity. That is clear evidence that this is a different kind of bacterium from those which both move and thrive in Pasteur's solution. You will observe, also, it is somewhat peculiar in the form of the segments; they are oval, and not so rod-shaped as bacteria generally. These you will always find in milk when it is in the early stages of souring. This is a flask of boiled milk, prepared on August 27th. It has not coagulated; it has undergone none of the changes to which I have alluded. There has been no butyric fermentation, no oïdium lactis has formed upon it, no putrefaction. This milk is as sweet as when it was first prepared. From this same flask, with precautions with which I will not detain you, I have charged various glasses. This has been charged for weeks: the milk remains fluid, you observe, although there is abundantly free access of air. The oxygen of the air and the caseine which still exist in the boiled milk have together been unable to bring about this lactic fermentation. As regards boiled milk, we have already sufficient evidence that the lactic fermentation is not something to which the boiled milk is spontaneously prone; it requires something to be introduced into it from without. Not only so, but, suppose you take a series of glasses of boiled milk like these, and introduce into them a series of drops of water, you will get fermentation in them. If you take, for instance, a drop as large as a quarter of a minim, you will have a fermentation in every one, and an organism in every one; but you will neither have, according to my experience, the lactic acid fermentation nor the bacterium lactis. You will have bacteria of other sorts, fermentation of other kinds.

Again, suppose you take a series of such classes, take off the glass shades and the glass caps, in different apartments and at different times, and expose the milk to the air-dust for half an hour; you will get fungi and bacteria of different sorts, but you will not get the lactic fermentation. According to my experience, you will not get the bacterium lactis; and thus it turns out, so far as boiled milk is concerned at all events, that the ferment that brings about this particular fermentation is a rare ferment. So far from boiled milk being spontaneously prone to the change, it requires something to be introduced from without, which is a rarity both in ordinary water and in ordinary air, and yet, undoubtedly, this is a true fermentation.

If you were to take such a glass as this, and dip the point of a needle into a glass of souring milk, and touch with the needle point the edge of the milk, within two or three days it would be a sour clot. Then you should find, as certainly as I did, the bacterium lactis throughout the mass.

But then, it may be urged, indeed such arguments have been used, this may be very well for boiled milk, but how about unboiled? May it not be that, by boiling the milk, you have destroyed certain chemical substances, purely hypothetical we must admit, but which we do think likely to exist? It is very difficult sometimes to avoid an offensive expression. I do not at all wish to make use of one; but it may be, according to the views of some persons, that in the unboiled milk there may exist certain chemical substances prone to evolve into organisms by spontaneous generation, and prone to produce these and other fermentations; but which, by the act of boiling, we deprive of this tendency; and, therefore, with a view to meet this objection, the first part of my investigation was devoted to endeavouring to see whether or not milk, as it comes from the cow, really does or does not contain materials tending to the development of organisms or to fermentation of any kind·

An exceedingly simple experiment will probably serve to convince you to a considerable extent with regard to this matter. If you go to a dairy where there is also a cow-house. take a couple of clean bottles, and fill one with milk from the dairy and the other with milk direct from the cow in the cow-house, the milk obtained from the dairy will be certain to sour, but the milk that you get direct from the cow will very probably never sour at all. It will acquire a nasty bitter taste, and will not have the bacterium lactis or the oïdium lactis, but some other kinds of fungi. Well, now, a very simple experiment is enough to show that the lactic acid fermentation is not a change to which milk is spontaneously prone. If this be so, it occurred to me that, if we were to take more care in the experiment, and also to perform the experiment so as to take the milk into vessels previously purified, in small quantities, we might be able to get the milk not only without the lactic acid fermentation or the bacterium lactis, but without any fermentation or any bacterium or any sort of organism. Accordingly, I arranged a number of little glasses like these, and little test-tubes with test-tube covers arranged upon a stand of glass-tube and silver wire. These were put into the hot box and heated to 300 deg. Fahr., and then, some milk having been received from the cow into a vessel purified previously by means of this pipette attached to a syringe, the pipette having been purified, milk was drawn up into the pipette, and then, by means of the syringe, each little

cap being in succession raised, a few minims of milk were introduced into each of the glasses, the caps being immediately reapplied. The result was, every one of the milks underwent fermentations, every one of them; and every one of them contained organisms, some of them as many as three different ones. The great majority of those twelve glasses which I used presented little orange specks, such as were never seen, I suppose, in any milk before; and, on examining these, I found them to be little organisms, to which I have ventured to give the name granuligera, because they consist of granules, different from bacteria in this respect, that you might suppose them not to be organisms at all till you had the opportunity of seeing them undergoing multiplication by fissiparous development, multiplied by crosswise fissiparous development; in a manner, however, differing from the transverse fissiparous multiplication of bacteria, in being crucial. But, besides the granuligera, there were bacteria of different sorts, and also some torulæ, but there was no bacterium lactis, and there was no lactic acid fermentation. What inference were we to draw? Was I to suppose that, although the lactic acid fermentation had been excluded, it was impossible to exclude others; that others were present in the milk as it existed in the cow's udder; or was it that I had not been sufficiently careful? The latter was the view I was disposed to take. The experiment had been performed in the cow-house, where certainly the air might be supposed to be reeking with organisms. I, therefore, performed the experiment a second time, and this time in the open air. It must be confessed it was not far from the cow-house, and it was a fine day at the very time of the year in which organisms most abound. On this occasion, I had twenty-four of the little glasses which you see before you. This time, again, every glass had organisms in it. At the same time, every glass seems to be different from all the rest. Such fermentations as there are here, I venture to say were never seen anywhere before. I have brought before you a diagram, showing some of them on a large scale. I want particularly to direct your attention to these strange scarlet spots which occurred in almost all of them. They began in tiny scarlet dots, which spread as fermentative changes, and, therefore, capable of self-multiplication in the substance of the milk. Here is one that is green, and here is another of an orange yellow colour. Here are two that have two kinds of filamentous fungi. I have not examined the species, but I shall very likely find that it is some species that has not been described. I found in the first experiment, as one of the organisms, a filamentous fungus of the most exquisite delicacy, though in general type of the same sort of arrangement as the common blue mould or the oïdium lactis. The size of the

filaments was so exceedingly small that twenty of them would lie abreast in a single human red corpuscle; they were smaller than even the bacterium lactis, smaller than the majority of bacteria. I believe no such exquisitely delicate filamentous fungus has ever been seen, even by Professor Bentley.

Now, how are we to explain this strange result? I am still disposed to believe that these organisms have got in for want of sufficient care on my part. But how are we to explain these strange appearances? Simply thus. If the bacterium lactis had been here, it would have taken the precedence of all other organisms in its development and the changes which it would have introduced would have made the milk an unfit soil for these organisms. Therefore I said—perhaps you might think me rash in saying so—there never were such fermentations or such organisms seen before in milk, simply because they never had the chance that they had here of coming forward; they would have been smothered—killed—by the effects of the bacterium lactis and other bacteria. Well, I determined to make one more attempt. This time I took the original twelve glasses. I mentioned that a large proportion of these glasses had scarlet spots, but I have not yet investigated upon what organism they depend. In the former experiment in the cowhouse, the great majority had orange spots, and those, as we have seen, were composed of heaps of granules. It occurred to me that one cause of failure might be this. Suppose one single group of these granules to exist, and to become disturbed and broken up, it might vitiate the whole specimen of milk; therefore, instead of drawing up the milk into the pipette with a syringe and then expelling it, I determined to have it introduced as directly as possible into the little glasses. These glass vessels having been purified by heat, the piece of India-rubber connecting them could not be purified by a very high temperature; it was purified by boiling. Cotton caps were tied over the ends of the glasses during the heating. The cow was taken out again into the open air, and this day the elements were in my favour. It had been a drizzly morning, and I might fairly hope that some of the multitudes of organisms existing in the little orchard might have been washed down and that the air might have been purified. I was also more careful in this respect. I got the dairy-woman to milk the cow without drawing the hand over the teat, so that the end of the teat should always be exposed by an action of the fingers in succession. Her hands were washed with water, and the cow's udder also, and she having squirted a little milk to wash away the organisms from the orifice of the duct, the cap was removed and the end of the tube was held in the immediate vicinity of the teat; a few drachms were introduced, then the cap was readjusted, and

then these little glasses were filled by the simple expedient of alternately relaxing and compressing with the finger and thumb on the caoutchouc, so that there was as little disturbance as possible of the organisms that might be supposed to be introduced in spite of my care. It is six weeks since this was done. At first sight, you might suppose, contrasting these appearances with those of the other tubes, which were only filled three days earlier, that the latter milks were all pure. The truth is, all but two have organisms in them; but I may mention that all but four had obviously organisms in them before I went for my trip on the Continent three weeks ago. In the course of the three weeks that have elapsed, two others have gone; but they already showed organisms which, though very pale and insignificant, were quite easily seen by a magnifier in such considerable mass that I felt sure they must have already been growing for a considerable time; and, therefore, in all probability those that still seemed to the naked eye and to the magnifier free from organisms were really so. Accordingly, two days ago, I drew out milk from one of those that seemed to be still pure, and I had the great satisfaction of finding the milk not only perfectly fluid and tasting perfectly sweet, with a perfectly normal reaction, purpling both the blue litmus paper and the red litmus paper—the normal reaction of perfectly fresh milk—but under the microscope I could not discover any organism of any kind whatsoever. Therefore, I think we are justified in saying that in unboiled milk as in boiled milk, provided, of course, the cow be healthy, there does not exist any constituent having any power of giving rise to organisms or producing the lactic or any other fermentative change.

This is the first step; the second will not occupy us so long; and I must beg that you will give me the opportunity of mentioning it, because, I believe, you will agree with me that it is the far more important step of the two.

The second part of the investigation was to find absolute evidence, if possible, whether the bacterium lactis was or was not the cause of the lactic fermentation. It occurred to me that, if we could estimate with some degree of accuracy the number of bacteria present in a given quantity of the liquid, and then if we were to dilute the milk with a proportionate quantity of boiled water, we might have the diluted milk so arranged that every drop with which we should inoculate boiled milk might contain, on the average, one bacterium; and if we should do so, as it would be practically certain that they would not be distributed with absolute uniformity, we should expect that we might have as the result of these various inoculations, some glasses with the lactic fermentation, some glasses without it, some with the bacterium lactis, and some without it; and, if it

should turn out that we should get those glasses with the bacterium lactis which underwent fermentation, and, on the other hand, those glasses which had no fermentation free from the bacterium lactis, that would prove the point, as, I think you will agree with me, it did, when we come to discuss the matter at a little more length after we have all our facts. Well, how are we to determine the number of bacteria existing in the liquid? This was done in a simple manner; a little covering glass, just half an inch in diameter, was used. Of course, we know how many square thousandths of an inch there are in the area of this little glass. We also know by the micrometer how many thousandths we have across our microscope, and, therefore, by calculation we know how many square thousandths there are in our field, and thus we can tell how many fields there are in the covering glass. I also, by means of this little syringe with a graduated disc and a piston rod in the form of a screw, graduated 2-100ths of a minim, by which means you can, with perfect precision, unit 1-100th of a minim, or 2-100ths, or any number you choose. I found that 2-100ths, or 1-50th, exactly filled the covering glass, so to speak; so that, when put down upon a glass plate, the rim of fluid round about the covering glass was not one-quarter of the diameter of the field; using the highest magnifying power, so that practically it was all under the covering glass. I knew, therefore, that there was 1-50th of a minim under the covering glass. If, then, I counted how many bacteria there were in a field, and took the number of the different fields and struck the average, I found how many bacteria there were on the average in the field; therefore, by calculation, how many there were under the covering glass, and how many there were in the 1-50th of a minim; and, consequently, I knew how much boiled water I ought to add in order that the drop of whatever size I might wish it to be should contain, on the average, one bacterium, and one only. This being done with a particular specimen of souring milk, I found that it was needful to add no less than one million parts of boiled water to the milk to ensure that there should be rather less than one bacterium, on the average, to every drop. Then, with these inoculating drops I inoculated five glasses of boiled milk; and the result was that out of the five only one curdled; but one did curdle, and that one had the bacterium lactis in abundance; the others did not curdle, underwent no fermentation whatsoever, and had no bacteria in them. You may say, perhaps, "How was it that there were none of these different things that you have been showing us?" Simply for this reason, that although these existed, and one of them existed probably in every two or three minims of the milk, yet they were in exceedingly small proportion to the

bacterium lactis, so that you might have searched, perhaps, for a whole day, with a high power of the microscope, and never discovered one. We are apt to forget how difficult it is to find these minute objects, unless they are very numerous indeed, in liquids. Therefore, when we came to dilute the milk with a million parts of water, the chances of getting anything but the bacterium lactis were exceedingly small. It was with reference to the bacterium lactis that the dilution had been made, and not with reference to these other organisms so exceedingly small in quantity. It so happened that we saw in the souring milk that there was another kind of bacterium, a moving kind different from the bacterium lactis; it was in every field, but not nearly so numerous as the bacterium lactis; it did not, however, exist in the one milk that curdled.

Now, having the bacterial ferment pure, we had the opportunity of performing other experiments; and the last experiment that I shall mention is this. These five specimens which you see before you were inoculated each with a drop calculated to contain two bacteria; these other five were inoculated each with a drop calculated to contain one bacterium; these five open glasses were also inoculated with drops each calculated to contain one bacterium; and one with a drop calculated to contain four bacteria. The result was that the specimen with the drop calculated to contain four bacteria curdled in a few days; and all these five calculated to have two bacteria to a drop curdled in a few days. The milk, you see, is perfectly solid, and curdled and soured. You will observe that no other change has taken place except the lactic fermentation, no oïdium lactis, and no other alteration; it is as pure in whiteness as when it was first coagulated. I may here mention that, although all these coagulated, they did not all coagulate at the same time. There was a time in the twenty-four hours during which the coagulation went on, in which I hoped that some of them were going to be permanently fluid; implying, as you would expect, that the particles of the ferment were not uniformly distributed; some had more than others, though each happened to have one. But, of the five glasses calculated to have one bacterium to each inoculating drop, three have remained fluid, and two of these others; so that, of the ten, exactly five, it so happens, have remained fluid without any curdling. I may consider myself somewhat fortunate that I have succeeded in bringing these all the way from Edinburgh in this condition. I will now deprive this of the protection in which it has hitherto lived. [Professor Lister poured out some of the milk and drank it.] It is perfectly sweet. It has a slight flavour of suet, which M. Pasteur has described as resulting from the oxidation of the oleaginous material of the milk.

Let me note this curious circumstance, that, of those specimens which did coagulate, those in the tubes coagulated considerably earlier than those in the more open vessels. At first, it seemed as if, for some strange reason, they were going to remain more fluid than those in the open vessels—even those that had, according to the calculation, four bacteria to the drop. I believe this is to be explained on the same principle as Pasteur has explained a corresponding fact with regard to the yeast-plant. He has shown that, if a saccharine solution be put in a very thin layer in an open vessel of yeast, the yeast-plant developes to a very great extent, but very little fermentation occurs; on the contrary, if it be put into a deep vessel, the development of the yeast-plant does not go on so rapidly, but more fermentation occurs. He explains it this way: that the yeast-plant requires oxygen for its nutrition; if it get it easily, as it does in a shallow vessel in the air, it produces comparatively little effect in breaking up the sugar into its constituents. So here, in the deep vessel, the carbonic acid accumulates, supposing any to exist, as in a well. Here the bacterium lactis had but little opportunity for getting oxygen. Accordingly here, just as in M. Pasteur's experiments with the sugar, the lactic ferment produced more rapidly its effect of lactic fermentation.

But this, you say, is assuming that bacterium lactis is the ferment. Now we are coming to that point. I say that this fact demonstrates, as I believe, that the bacterium lactis is the cause of the lactic fermentation. I must add one point by way of fact. For the satisfaction of others rather than for my own, I went through the laborious process of investigating portions of all these vessels, and I found that, in every one in which the lactic acid fermentation had taken place, where there was curdling and souring, the bacterium lactis was present; and in no instance in which there was no lactic fermentation was any bacterium of any sort to be discovered. I believe that demonstrates that the bacterium lactis is the cause of this very special lactic fermentation. But, let us assume for a moment that there did exist some other particles besides bacterium lactis in the milk capable of causing a fermentation; that the lactic ferment were not the bacterium at all, but some chemical ferment. First of all, you will please to observe that we have from this experiment absolute evidence that the ferment, of whatever nature, is not in solution, but in the form of suspended insoluble particles. If the ferment had been in solution, every drop would have produced the same effect. The fact that some drops were destitute of the ferment proves that that ferment was not in a state of solution. That is absolutely demonstrated. Now, suppose we admit, for the sake of argument, that the lactic acid ferment was some non-living substance, capable of

self-multiplication as rapidly as the bacterium, but not living; a strange hypothesis, no doubt—but suppose we admit it. Suppose we admit that these chemical ferments, the chemical lactic ferment and the bacterium lactis, were merely accidental concomitants of each other, it would be absolutely inconceivable that these two accidentally present things should be present in exactly the same number. But, suppose you admitted that—that there were exactly as many of the bacterium lactis as there were of the ferment, the true fermentative particles—suppose you admitted that inconceivable thing, I say it would be again inconceivable that they should accompany one another in pairs, that invariably where there was bacterium lactis there should be a ferment particle, and where there was no bacterium lactis no ferment particle. That would be as inconceivable as the other. Therefore, we have two inconceivables, one of which would have been sufficient to show that we cannot admit any other hypothesis than that bacterium lactis is the cause of the lactic acid fermentation.

But the experiment tends to even more than this. Where we find the effect so exactly proportioned, as regards the number of glasses affected with fermentation, to the adult bacteria that we count, we are led to infer that this particular bacterium, at all events, has not any spores—that there are no spores existing in addition to the bacteria. People seem often to assume that these bacteria must necessarily have spores or germs. It seems to me an unlikely thing that they should. If they are, as it were, a generative apparatus *per se*, they are constantly multiplying; why should they have spores? I do not say that bacteria may not have spores. There are very different kinds of bacteria; some may have spores, and some may not; but this sort of result seems to indicate that this particular bacterium has no spores; because, if we had, besides the bacteria that we can count, spores of bacteria disseminated through the liquid also, we should have the effect more than in proportion to those bacteria that we have. The only fallacy here is that it may be that the bacterium has not been diffused uniformly through the milk. Therefore, I do not say that in this case it is absolutely proved yet. At all events, this experiment gives us a line of inquiry, by means of which we may probably settle that point with regard to any individual case of bacterium. This, however, I do not desire to urge upon you; but, what I do venture to urge upon you, is that you will seriously ponder over the facts which I have had the honour of bringing before you to-day; and, if you do so, I believe you will agree with me that we have absolute evidence that the bacterium lactis is the cause of this lactic acid fermentation. And thus I venture to believe that we have taken one sure step in the way

of removing this important but most difficult question from the region of vague speculation and loose statement into the domain of precise and definite knowledge.—*British Medical Journal, Oct.* 6, 1877, *p.* 465.

28.—OAKUM AS AN ANTISEPTIC DRESSING.

By Dr. Robert Esler, M.Ch.
(Read before the Ulster Medical Society.)

The value of oakum as an antiseptic dressing was first brought under my notice by the late Dr. John M'Crea, who had been for some time using it extensively; and to this, as to all other subjects which he took up, was applying himself assiduously in observing, and noting carefully what he observed, as the result of his experiments. Hence it is more as a tribute to the memory of my late friend that I come before you with these few and imperfect notes, than from any claim I have from my own brief experience, to ask you to take my opinion for anything but what it is worth. At the same time I hope to be able to put before you as many facts as will stimulate an instructive conversation.

Oakum, as you are no doubt all aware, is simply tarred rope reduced to its original state of flax or hemp, with the addition of the tar. Any of us who have been a voyage round the world, or even a shorter circuit, may have observed that the idle time of the sailor is often filled up by the occupation of oakum-picking; and in gaols, workhouses, and penitentiaries, it is made an occupation of profit. Old ships' rigging can be bought at about 17s. per cwt., and the oakum, when picked, sells at 22s. per cwt., so that the difference represents labour. The original use to which oakum was put was to caulk the crevices of wooden ships; but modern iron vessels, excepting the decks, do not require, nor will they be satisfied with, such a soft substance to mend their breaches—so that, I suppose, the primary use of oakum was to save life; but, more recently, it has been applied to life-saving in quite a different capacity. During the American war, carded oakum was employed extensively in the dressing of wounds; and, subsequent to that time, the surgeons to the Children's Hospital in Great Ormond-street, London, introduced it to this country. Early in the year 1870, the Lancet drew the attention of the profession to it, as used by Mr. Pollock, at the St. George's Hospital, and gave it as their opinion that it would supersede other methods, and one reason was its low price, quoting it, as prepared by Bell of Oxford-street, at 1s. per lb., but you will observe that the price which I have paid for it is a little more than 2¼d. per lb.

About the year 1870, Professor Lister put the antiseptic

qualities of oakum to the test, and found it to answer his expectations; his observations are recorded in the British Medical Journal of January of the following year. Trélat, a French surgeon, also used it some five or six years ago, and I suppose that, at the present time, it is to be found in the wards of most of the hospitals of this country; but it is because I do not think that its true value has been ascertained, that I wish to press its use upon your attention. Why it is in use in our hospitals, or by whom it was introduced, perhaps few of us have paused to inquire; but it may be enough for us to know that, having it there, it is a suitable agent, in many respects, with which to surround suppurating wounds. And we hope to be able to show that, as an antiseptic agent, it is superior to any other ready-made substance we possess.

My observations have been made first, in the Union Infirmary, and subsequently, but more minutely, in the Ulster Hospital for Children, and they have been in the following class of cases:—Amputations, abscesses, ulcers, cutaneous erysipelas, phlegmonous erysipelas, and burns. The number of cases of the first class in which I have used this dressing have been in two major and several minor amputations. Either of two methods may be adopted. In the first, the dressing is made as simple as possible, A single strip of lint soaked in carbolic or spirit lotion, applied next the wound, with a roll or two of bandage to hold it in position, and the stump then surrounded with oakum and another bandage if necessary, or you may apply the oakum next the wound, and this method is less objectionable than would at first sight appear; and, for reasons to which I shall presently allude, the flax may be much preferable to cotton in contact with a granulating surface. The advantage of this dressing is, that for stumps, it is simple of application, it makes a soft pad on which to rest, and, what is of the greatest importance in the wards of an hospital, *it effectually keeps down any offensive odour*—and more, its effect seems to be to destroy the putridity of the discharge, while the pleasant tarry smell is strongly suggestive of the sea, of ships, and of fresh tar. In most of the cases in which I have seen it applied, healing has taken place by the first intention, and this is, no doubt, partly accounted for by the freedom with which the serum is discharged from the wound, and by the prevention of ingress to any substance which would favour decomposition.

In a Chopart's amputation, in the Children's Hospital at present, you may see the result of this dressing. I am applying it next the wound; it has kept perfectly sweet. And this I attribute to the antiseptic properties of the oakum in which the stump was enveloped.

In *Abscesses*.—I have opened some dozens of abscesses where

the only subsequent treatment was a pad of oakum bandaged over the part; and, in every case where there was not diseased bone underneath, the result was more satisfactory than I have ever seen from any other treatment. I will instance one case, that of a labourer, aged about fifty:—The abscess was gluteal, and had grown to almost the size of his head; the skin and muscles were enormously distended. So tedious did I expect the healing that I thought of introducing a drainage tube or plug, but having faith in oakum, I applied a good large pad over the part after evacuating the contents, directed the man to lie on it, so that any discharge might drain into the oakum. The shrunken skin and relaxed muscle, in a few days, seemed to fit into their places, the opening healed without a blush of inflammation, and in a week the man was in his usual health. In some cases it is desirable to poultice for some time before using the knife, and we all know how troublesome it is to continue this for any length with linseed meal, and many surgeons have a great dislike to these oily applications. Liston said, nearly forty years ago, that "hot dressings, filthy unguents, greasy poultices, stimulating plasters, and complicated bandages must give place, very soon, to the elegant substitute for a poultice," referring to isinglass plaster, "and to careful position of the injured part;" and added that it was so long since he used these filthy compounds that his nurses had forgotten how to make them. Now in oakum we have the material for an antiseptic poultice, in abscesses or inflammatory attacks, by simply dipping it in hot water and covering it with water proof tissue.

In speaking of *Erysipelas*, I need not refer to the many specifics which, from time to time, are recommended and vaunted, nor to the difficulty experienced sometimes in controlling the disease, nor will I allude to the constitutional treatment, which should not, in any case, be neglected, but simply to the local application of oakum as a remedy, and I will instance a case or two, in both the cutaneous and phlegmonous stages, in illustration of what I have observed in treatment with this material. The first happened in the Accident Ward at the Union Infirmary, where a strong man, who had been recently admitted, presented one morning the characteristic appearance of the erysipelatous inflammation; the left side of his face, but especially his eye, was most effectually disfigured. I was accompanied by the house surgeon, to whom I said, "What would you do for this man?" His reply was, "Apply a pad of oakum." So I said, "A pad of oakum let it be." The following day there was scarcely a trace of the inflammatory action present, the swelling had subsided, and there was no return; the application was kept on, however, for several days.

A strumous boy had a true erysipelatous blush on the wrist of the left arm. Next day it had extended as far as the insertion of the deltoid; the limb was surrounded by oakum; the inflammation was arrested at the spot then affected; the arm got well; metastasis followed, however, and the face was attacked a couple of days subsequently. The same application brought about the same result, and I find that Dr. M'Connell is so well pleased with the line of treatment that, when I visited the Infirmary a few days ago, he had several erysipelatous patients encased in oakum. So much for the cutaneous variety. And in the deeper and more serious stage of the same disease—viz., the cellulo-cutaneous, where abscesses have formed, or sloughing is taking place, I am of opinion that oakum is simply invaluable.

The first case, in illustration, which I will mention, was that of an elderly man whose right leg was very extensively involved. The first abscess formed over the fibula, the next above the knee, and another in the vicinity of the groin. I used the knife freely; and, as I had before my mind's eye several cases of poulticing and extensive sloughing, I decided on enveloping the limb in oakum only. No other application of any kind was employed; the patient was anæmic, and the prognosis of the gravest kind. The effect of the dressing was to most thoroughly keep down any smell. Almost immediately healthy action set up, not an unfavourable symptom was present by the end of a week, and the recovery was rapid and satisfactory.

The second case was that of a female lunatic; the part affected was the left leg; the course pursued was the same; and, as far as the leg was concerned, the healing process went on, but bed-sores formed, and the drain on the system was so great that death resulted; but the comfort, the simplicity of the application, and the comparative absence of putrid odours, during a long illness, were results not to be lightly valued.

Oakum in Treatment of Burns.—I need not say a word, by way of introduction, as to the offensive nature of the discharge from burns and scalds, and any means of controlling or lessening this will be hailed by the surgeon and by his staff of dressers with pleasure. Now, I venture to state that one of the best agents we possess for this purpose is oakum; its value as an antiseptic is even more marked in this class of cases than in the former. And while I would rather not have a burnt case in an accident ward, yet, were I obliged to do so, I would simply surround the patient with plenty of oakum, make the dressings so thin and simple that the discharge would flow freely into the tarry mass; and by renewing this sufficiently often, the matter will be prevented emitting that characteristic odour of burns which is so overpowering, and the

labour of the dresser will be much lightened. I have thus treated about a dozen cases of burns, some of them very extensive, and in all the smell was so thoroughly kept under that the patients were not more to be dreaded in a ward than any other surgical case with an extensively abraded surface. Nor need there be any hesitation about applying it next the burn without any other substance intervening. And it does not seem to be so difficult to get it detached as lint; but surgeons' lint, in common use, which should be made of flax, is composed altogether of cotton. You know there is a popular notion that cotton is an exceedingly bad application to sores, that it causes great irritation, and that linen is preferable, and these popular notions are invariably based on correct observations. We may pause here for a moment to inquire is there any cause for this. When we place a fibre of linen and one of cotton under the microscope, we might infer that the cotton would be the less irritating of the two, as its fibres are not so sharp or well defined. Indeed the cotton, although flat, has a perceptible thickness at the edges, is rounded, and somewhat like a thin tube flattened; the flax is not so tubular, not so well defined or rounded, and is sharper; but if, after examination in the dry state, we apply moisture, the cotton fibre is found to twist in a spiral manner; and, perhaps, it is this hydroscopic power of twisting, when moistened, that gives cotton its irritating qualities, and the absence of this quality in the flax which makes it strip off granulating surfaces so easily.

To briefly sum up. The conclusions at which I have arrived, regarding oakum as a dressing, are that:—It is cheap; it is clean; it is easily applied; it is equal to any, and in many respects superior to most other dressings of the same class; and that it is antiseptic. I have not tried to demonstrate by experiment, apart from clinical observations, this last quality, but I intend to do so. And only that it is a wrong method to enter upon an investigation with a preconceived notion, I would state my conviction that it will be found to present a barrier to the ingress of spores or sporules, or whatever germ life infests the atmosphere, and which are such a bugbear to a large school of operating surgeons. Should this opinion prove to be correct, it will be just simplifying Lister's antiseptic method, the principle being exactly the same, as tar and carbolic acid stand side by side as therapeutic agents.

Tar is very complex in its composition; it is an altered resin and an empyreumatic oil, in which the following substances are found:—Creasote, paraffin, picamar, and eupion. It is obtained by the destructive distillation of the pinus sylvestris, and at gas works, which is a huge destructive process of the old pine forests, as they appear to us in the form of coal beds.

As a therapeutic agent, tar is used principally in chronic skin diseases; its influence on mucous membranes is sometimes well marked; and, in the form of inhalation, the oil of silver pine often given satisfactory results in chronic bronchial affections.

Tar water long enjoyed a reputation as a specific in numerous diseases; it is made by shaking up one part of tar with four parts of water. The celebrated Bishop Berkeley entertained the notion that tar was a certain remedy for *every* form of disease. And as the old philosophers cried aloud from the housetops to their fellow-citizens—"Educate your children," so the bishop said that if he had a situation high enough, and a voice loud enough, he would cry to all the valetudinarians upon earth—"Drink tar water." I would not be quite so enthusiastic regarding this tarry preparation, oakum; but, by this paper, my appeal to all who are practising surgery is—"Try oakum dressing."—*Dublin Journal of Medical Science, October* 1877, *p.* 324.

29.—FRACTURE OF BOTH BONES OF THE LEG, TREATED WITH A NEW BOX-SPLINT.

By Montgomery A. Ward, Esq., M.B., M.Ch., Univ. Dub.; Assistant Surgeon to the Adelaide Hospital, Dublin.

Every surgeon who treats fractures of the leg by any of the ordinary methods must remark the frequency with which patients complain of a pain in the heel, sometimes slight, but occasionally very severe indeed; and as it is the duty of the surgeon to remove every source of discomfort and irritation in the treatment of his cases—for a slight cause often produces severe and unpleasant constitutional disturbances, especially in persons of nervous temperaments or broken-down constitutions—I venture to bring before the profession a box-splint which I have devised to obviate this distressing pain in the heel so often complained of, but more especially so when the ordinary box-splint is used. For years I have remarked patients complaining of this pain, and the only methods I have ever seen attempted to relieve it—viz., by pads, French wadding, &c.—frequently give but temporary relief. I should mention that Dr. Hughes, Surgeon to Jervis-street Hospital, has improved Dupuytren's splint by adding to it a very ingenious mechanical support for the purpose of relieving the heel from pressure.

I had the misfortune to suffer from a severe fracture of both bones of my left leg, which laid me up for a long time, and I suffered more pain and annoyance, when in splints, from my heel, than from the fracture itself. Constantly I was kept awake all night by this pain alone, despite all the efforts used to alleviate it, while on all occasions it was a great source of

annoyance to me—indeed I would not have believed that this pain, which surgeons think so lightly of, was so distressing, until I suffered from it myself.

Case 1.—On July 2nd, 1876, I was requested by the late Dr. Thompson, of Bray, to see, along with him, a gentleman who had broken his leg in the following way:—As he was riding down a steep hill, in the vicinity of Bray, on a velocipede, he lost all control over the machine, and was dashed with great violence to the ground. Besides other injuries, both bones of his right leg were fractured, the tibia being broken in two places; the fracture was not compound, but there was a good deal of extravasated blood, indicating the rupture of a small artery. We temporarily kept the leg in position by two sand bags until a local carpenter made a box-splint. I, bearing in mind the pain in the heel, suggested that the under-splint, on which the leg rested, should be hollowed out for the calf of the leg, and a round hole cut in it for the heel; this was done. I saw this gentleman several times with Dr. Thompson, and had charge of him for a month during Dr. Thompson's absence in Scotland, and never once did he suffer from a pain in the heel, and eventually made a fine recovery. I was so pleased with the result of this case that I asked Messrs. M'Adams and Corcoran to make for me a similar splint, but better finished and more perfect than the rough and ready one made in a hurry by the Bray carpenter.

It will be seen by the accompanying illustration, marked No. 2, that the under or supporting splint consists of a double inclined plane, the thigh-piece, being hollowed out to support the convexity of the thigh, meets the leg-piece at a very obtuse angle, but just sufficient to cause perfect muscular relaxation; the under leg-piece is also scooped out to correspond to the calf of the leg, then elevated for the "tendo Achillis," and hollowed out again for the heel. A pad is made to fit accurately this under-splint—thus the leg can be kept in a perfectly straight line, the concavities of the splint being adapted to the convexities of the limb.

In the ordinary box-splint used in the Dublin Hospitals for fractures of the leg and ankle, as well as in Mr. Butcher's, the underpiece for the leg is perfectly flat; consequently, when a fractured leg is placed in either apparatus, more or less undue weight is thrown upon the heel, thus causing the heel-pain; to remedy this, surgeons are in the habit of placing extra padding beneath the tendo Achillis so as to take the pressure off the heel. This answers well in some cases, but I have frequently seen it fail; whereas in my splint no undue weight is thrown upon the heel, while every portion of the limb encased in the

box sustains the same uniform pressure. Again, the thigh-piece of the ordinary box-splint is flat, therefore not so comfortable, while in mine it is concave.

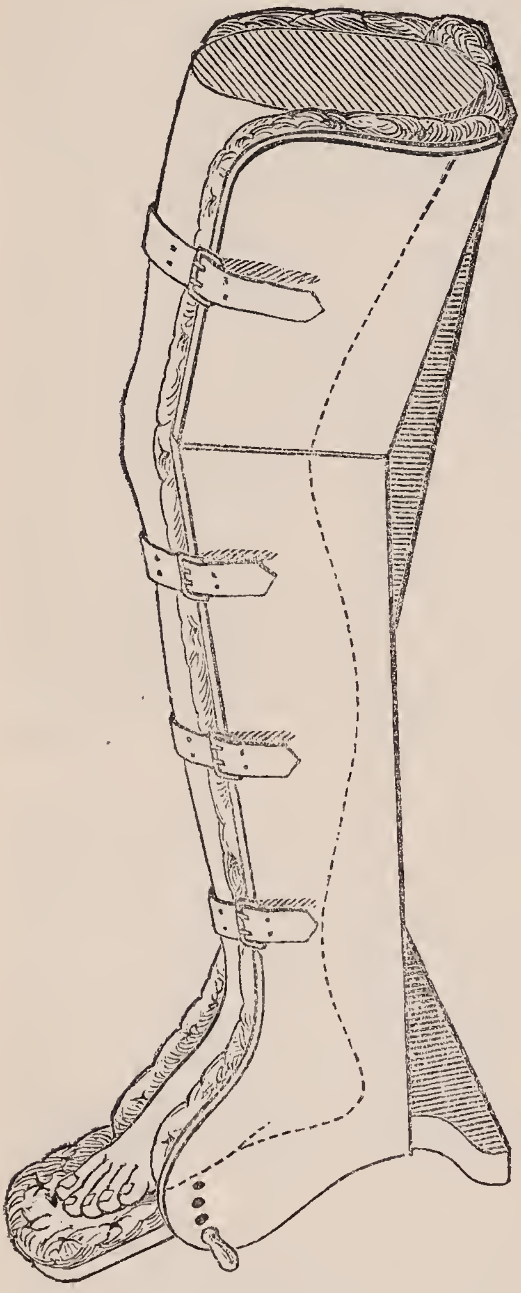


Fig. 1.

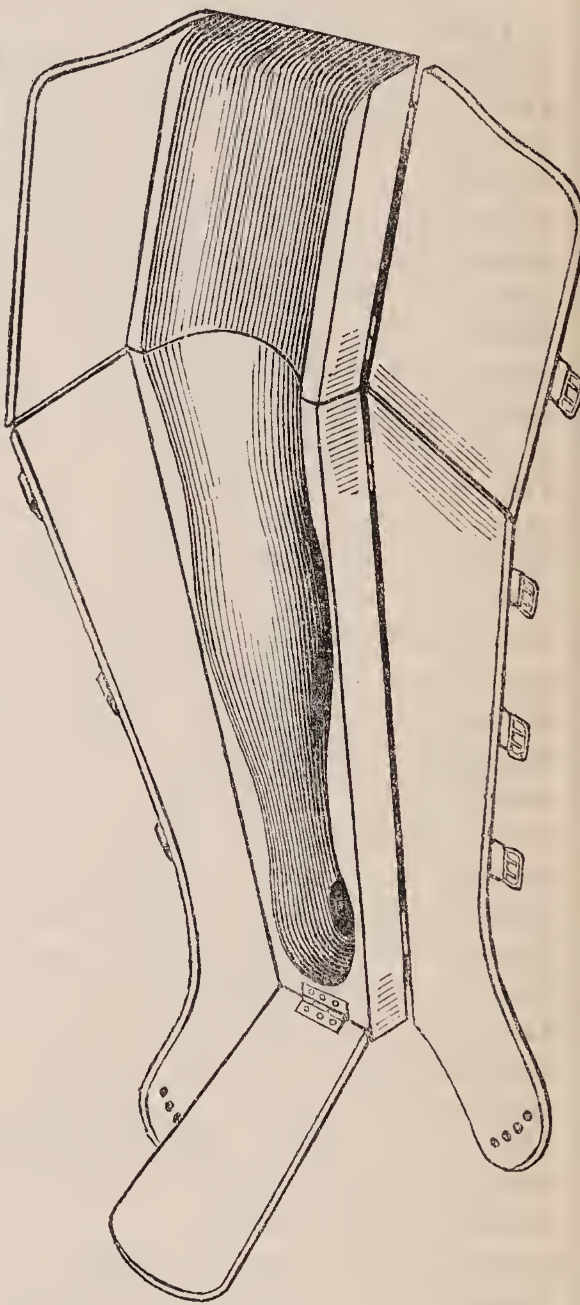


Fig. 2.

In the illustration marked Fig. 1 the straps appear pressing upon the thigh and leg, whereas in reality they do not touch it at all, as the sides of the splint are higher than the limb when *in situ*. Fig. 2 faithfully depicts the splint, with one side let down.—*Dublin Journal of Medical Science, Sept.* 1877, *p.* 205.

30.—ON DISLOCATIONS OF THE SHOULDER JOINT.

By Montgomery A. Ward, Esq., M.B., M.Ch., Univ. Dub.; Assistant Surgeon to the Adelaide Hosp.; Demonstrator and Lecturer on Practical Anatomy in Ledwich School, Dublin.

I have seen the various means recommended for reducing a dislocated humerus tried from time to time, and feel convinced that the easiest and most efficacious plan in the hands of a surgeon possessed of a moderate amount of physical strength, is the old method by means of the heel in the axilla. On one occasion I succeeded in reducing a subglenoid dislocation of the humerus of over two months' duration by this method, when all other means, including the pulleys, had failed, and just as all attempts at reduction were about to be given up as useless, the patient having been under the influence of chloroform for nearly one hour. In this case, no doubt, the previous traction of the pulleys, as well as the chloroform, facilitated the reduction. On another occasion, assisting Dr. Albert Walsh, I reduced a subspinous dislocation of the humerus, of six weeks' standing, in a powerful man, a commercial traveller, by the same means, while he was under the influence of chloroform. Lately I have adopted a plan as an adjunct to this method, in subglenoid and subcoracoid dislocations, which I first saw used by Dr. Walsh. It is extremely simple, and as I cannot find it mentioned in any text book on surgery, or surgical dictionary, with which I am acquainted, I shall briefly describe it. The patient must be placed in the usual position for effecting the reduction by means of the heel in the axilla, then pass a towel (a diaper one, if possible) round the arm, and make an assistant use traction in the direction indicated in the accompanying illustration. I believe this traction greatly facilitates the reduction by raising the humerus out of its new bed—in fact, it is a powerful lever of the first order, the fulcrum represented by the towel being close to the resistance, while the operating surgeon represents the power, his heel acting as a wedge. I have never failed in reducing a recent dislocation of the shoulder by this plan, neither have I used chloroform for the purpose of causing muscular relaxation, and, as some patients strongly object to being put under its influence, I believe this to be the safest and most expeditious method of procedure, in such cases, for the purpose of overcoming the resistance of the muscles.

I shall now give the particulars of three cases in which I adopted this plan with marked success, without the aid of chloroform:—

Case 1.—On February 4, 1876, I was sent for to see Mr.——, whom I was told had sustained a severe injury from a fall on the flags, while under the influence of drink. His friends told

me that his arm was broken. Accordingly I went, bringing with me the suitable appliances for a broken arm. [I may here mention that it has often struck me as remarkable that a patient suffering from a luxation of the shoulder, or his friends, constantly mistake this injury for a broken arm.]

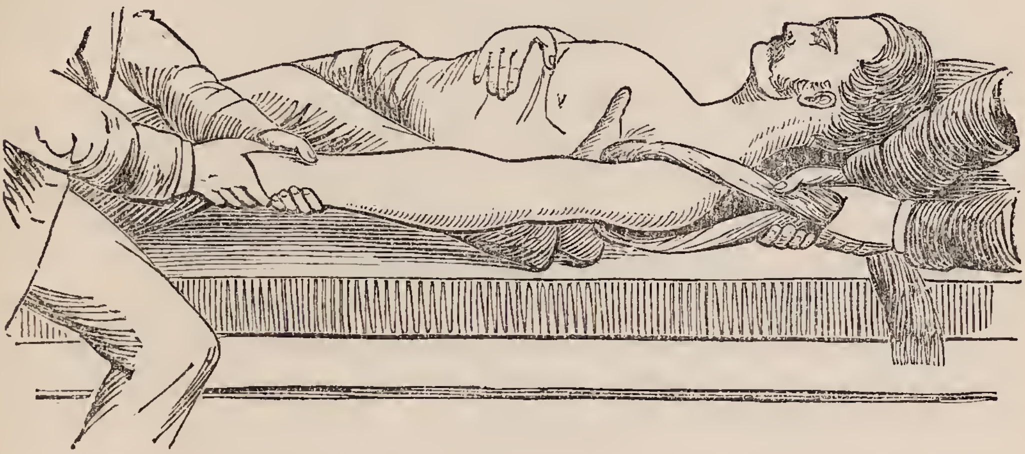
On arriving at the gentleman's residence, I at once ascertained that the arm was not broken, but that he was suffering from a subglenoid dislocation of the humerus. He was still under the influence of drink, being in a very nervous condition, and extremely troublesome. I attempted to reduce the luxation with my heel in the axilla, but failed, in consequence of the patient becoming very boisterous and unmanageable. I then essayed the plan with my knee in the axilla, also manipulation, but failed from the same causes. I now sought and obtained the assistance of my friend Dr. Walsh, who kindly came at once. Dr. Walsh passed a towel round the arm, in the manner before described, while I planted my heel in the axilla, and, notwithstanding the struggles of the patient, in a very few moments the dislocation was reduced.

Case 2.—On July 25, 1876, I was sent for in a hurry to see Mr. B., whom I was told had sustained some severe injuries in the following manner :—He was driving in the park with his brother and two ladies, when the horse took fright, ran away, and the carriage, coming into collision with some obstruction, was upset, and the whole party thrown out on the road. They all fortunately escaped with some scratches, with the exception of Mr. B., who, in addition to having received some severe contusions, sustained a subglenoid dislocation of the shoulder. As he was a very muscular, well-developed man, and as he stated that he would not on any account take chloroform, I thought that I might possibly have some difficulty in replacing the humerus single-handed, especially as I was obliged to use my left leg (which was a little weak, in consequence of a bad fracture). I asked for assistance. Dr. B. F. M'Dowell, of York-street, was sent for, and came immediately. I first attempted to reduce the dislocation with the heel alone, but did not succeed; Dr. M'Dowell then passed the towel round the arm, and I easily effected the reduction with my heel in a few minutes.

Case 3.—Lately I was sent for to see a lady, who, falling on her shoulder in the street, sustained a subcoracoid dislocation of the humerus. I reduced the displaced bone without any difficulty in a few moments with my heel, while her husband used traction with the towel under my directions.

In conclusion, I wish to state that I do not claim to be the originator of this plan, but merely desire to bring it under the notice of the profession, inasmuch as it has been so successful

in the practice of others, as well as mine own, and because I do not think that it is very generally known.



In this illustration the artist could not reproduce the exact direction of the auxiliary force which I wished to convey, as if he did so, to use his own words, the drawing would be "fore-shortened." The direction of the force indicated by the towel should more nearly approach a right angle than it is represented to do.—*Dublin Journal of Medical Science, Sept.* 1877, *p.* 211.

31.—HODGEN'S SPLINT, AS USED IN GUY'S HOSPITAL.

By J. Farrant Fry, Esq., L.R.C.P. (late House Surgeon.)

For some time past, fractures of the femur (whether of the neck or shaft) have been treated in Guy's Hospital with the Hodgen's splint. The results have been so uniformly satisfactory, that an account of the splint cannot but be of service, and will, in all probability, lead to its more frequent use.

A pole, about seven feet in length, is fastened to the foot of the bedstead, not at a right angle to it, but forming, with it, an angle of about 115 deg.; and, to make it more secure, it is better that the end of the pole should be fixed in the ground by a spike. To the upper end of this pole a pulley is attached. A piece of strapping (of which Leslie's brown holland is perhaps the best) is placed along each side of the limb, leaving a stirrup projecting beyond the foot, in which a piece of wood is placed, having some cord fastened to it. Thus vertically placed, strapping is now securely bound to the leg by horizontal pieces, extending from the ankle to as near the seat of fracture as possible, but, of course, omitting the knee.

The splint itself consists of an iron rod, extending on each side of the leg, from the groin on the inner and rather higher on the outer side, to a few inches beyond the foot, where

the two rods are united by a transverse one; the two pieces are further fastened together by two or three rods arching across the limb. The cord fastened to the stirrup is now tied securely to the transverse piece, bringing these two almost close together.

To support the leg in the splint, something must pass between the two rods and beneath the limb. For this purpose, strips of bandage may be sewn to the rod on one side, and each separately carried beneath the limb and fastened to the rod on the other side, after being drawn sufficiently tight to fit the inequalities of the leg. A cord is now fastened by its ends to the outer and inner rods, rather above the knee, leaving a large loop, at the centre of which is a double-block pulley, round the lower wheel of which the cord runs. Another cord is fastened in a similar manner midway between the knee and ankle, and leaving, like the first, a large loop, passing through another double pulley. The splint containing the limb has now to be raised from the bed by a third cord, which passes through the upper wheel of the two double-block pulleys, and thence round the pulley attached to the pole at the foot of the bed.

By this means, the limb can be raised to any height, the most convenient being an angle of about 135 deg. with the recumbent body. The lower end of the bed should be raised on blocks, to prevent the patient from being dragged down by the extension which is exerted.

Until this splint was introduced, the long outside splint with weights was generally employed; but whether this plan, the perinæal band, or double inclined plane, was adopted, the results were so bad that Mr. Cooper Forster, in the Guy's Hospital Report for 1875, says: "If an adult fractures his thigh, it is all but certain that there will be shortening of an inch; and that the method adopted to keep up extension does not matter, provided always that, whatever the plan may be, it is overlooked with care, the result will be about the same."

The advantages claimed for the Hodgen's splint over others are: 1. Less shortening of the limb; 2. Greater comfort to the patient, who is able to sit up in bed, to turn on either side, and conveniently to use the bed-pan; 3. Less liability to bed-sores in aged people. In fact, there is no one point in which (for an adult) any other treatment is to be preferred.

The results, as regards shortening of the limb, may best be shown by giving statistics of an equal number of cases. Out of seventeen unselected cases of fracture of the shaft of the femur, treated in 1874 by the long outside splint and weights, varying from ten pounds to fourteen pounds, the average amount of shortening was more than an inch; in one case, it was two inches; in five, it was one and a half; in five it was one;

and in two only, was it less than an inch. No case is recorded in which there was no shortening. This exactly accords with eleven cases reported by Mr. Cooper Forster in the Guy's Hospital Report for 1875. Out of seventeen also unselected cases, treated with the Hodgen's splint, the average amount of shortening was considerably *less than half an inch.* In two cases, it was one inch, and in all the rest it was less than an inch; six cases being returned without any shortening.—*British Medical Journal, June* 23, 1877, *p.* 772.

32.—ON THE TREATMENT OF ANGULAR CURVATURE OF THE SPINE BY THE PLASTER-OF-PARIS BANDAGE.

By Richard Barwell, Esq., F.R.C.S., Surgeon to Charing-Cross Hospital.

Angular curvature of the spine, always a result of caries in one or more vertebral bodies, is among the most intractable maladies with which the surgeon has to do. The cause of this intractability, not the origin of the disease, is that which more especially interests us now. When, then, from any cause one or more of the bodies have been partially eaten away on their front surfaces by ulceration, the column above this point inclines very considerably forward; the weight of the body is thrown on the anterior aspect of the bones, in the retreating angle formed by the bent spine—that is to say, at the point of disease. This causes increase of pressure on the ulcerating bone, and I need hardly point out that any undue pressure at an already inflamed spot favours and increases the tendency to caries. Besides mere weight of body there is another cause of pressure—namely, the contraction of abdominal muscles, which in nearly every case of angular curvature may be felt hard and contracted beneath the skin. This condition is, I believe, produced by irritation of the nerves as they pass from the interspinal foramina. Moreover, movement, even the very slight movement that takes place between the vertebræ, helps very materially to keep up diseased action. These three conditions cause the the inveteracy of curvature.

The natural, and, indeed, the only cure of vertebral caries is by anchylosis, which means that new osseous matter, thrown out by the adjacent healthy or somewhat irritated parts, binds together and consolidates the whole number of diseased vertebræ into one compact mass of bone. This new matter is not, of course, formed from parts actually ulcerating, but by the neighbouring periosteum either over less inflamed parts of the carious bones or over neighbouring bones sufficiently near to be irritated by the disease, or from both such portions of membrane. Also it must be observed that the districts which,

when inflamed, are destined not to ulcerate, but to produce fresh bone, must be in such position as to be subject only to little pressure; otherwise they might easily overstep the quantity of action necessary for a formative, and pass to that which leads to destructive processes, like the original malady. Such places are, during the continuance of the disease, generally at the sides of the vertebræ; hence there results first in that part a series of arches and buttresses which unite the healthy bone below to the next sound one above, while any still healthy parts of an intermediate carious bone throw out struts and spandrils to join the newly-formed bridge.

As, however, to the production of this new osseous structure absence of strong pressure is necessary, so for its solidification and union with the older bones considerable immobility is essential; for the material, which afterwards becomes bone, is, when first formed, not a hard substance, but a soft easily-compressible tissue, which moulds itself to the morbid shape of the column, twists or bends with each movement of vertebræ, and changes with the increasing angle of the spine, until it is hard enough to preclude motion and strong enough to prevent further collapse. This leads us naturally to perceive how the production of anchylosis should prove curative to the caries—viz., by the entire prevention of movement and of pressure. It also furnishes us with the necessary guide for treatment, if only it could be successfully followed—viz., to imitate nature in preventing both these conditions.

We thus come to consider two principles of treatment which have at different times found favour. The one is directed to making the ulcer as small as possible, by allowing collapse of the vertebræ above and below; the other aims at keeping those bones asunder. The former of these, enjoining rest, chiefly by means of recumbency, obviates that part of the pressure which is caused by superincumbent weight; but the other part, that by muscular action, is ignored. One sees such patients worn with long lying, bending up more and more, till at last those who get through the treatment rise from their beds with the deformity as fully developed as the circumstances would permit. What a weary matter it is, too, this condemnation to a bed or couch for years, one not at all likely to promote that state of health which is needed for the cure of strumous ulcerations, as well as for the establishment of such sthenic actions as might promote anchylosis.

The other method—that which aims at minimising the deformity by keeping the bones apart—effects at the same time diminution of pressure, especially at the front edges of the vertebræ. For a great many—indeed, for several hundred years, mechanisms for stretching the spine have been alternately

vaunted and discarded. Mr. Chessher, about sixty years ago, used to stretch his patients by means of a sort of rack, and fixed on the trunk, while thus elongated, a machine not unlike the spinal support of the present day. He failed to do much good, not, I believe, because the idea was faulty, but because all these mechanisms are nearly as futile for angular as they have been shown to be for lateral curves. They do, indeed, succeed for a time in taking some of the weight from the spine, by upholding the shoulders, but the mechanism soon gives and gets out of gear—it cannot fit close enough on the pelvis, &c. to support any great amount of weight. Nevertheless, as some means to keep the the spine from collapsing (the characteristic of this treatment) must be used, a great many modifications and changes have been introduced, which fail to do more than show the unsatisfactory nature of the whole machine.

But, as I have said, the idea is correct. Straightening the spine so as to annul pressure is the first step; but how to keep away pressure by some really effectual yet light appliance has been the difficulty, and this has been solved by Dr. L. Sayre. The plan is simple. The patient is to be suspended by the axillæ and head for a few minutes, while a plaster-of-Paris bandage is put on rapidly and allowed to harden. During this suspension, the weight of pelvis and lower limbs straightens to a certain degree the spine, and then the plaster, when the patient is allowed to stand, prevents the figure falling forward again; thus keeping the retreating angle open and obviating pressure on the front of the vertebræ.

The difficulties are these. 1stly, to render suspension innocuous and painless; 2ndly, avoidance of injurious pressure on bony prominences by the plaster; 3rdly, to make due allowance for respiratory movement, and yet to apply the bandage sufficiently firm to give full support.

The first of these embarrassed me for a considerable time, until, by using a steel cross and causing the padded straps to cross each other, I obtained a means whereby pressure on the axillary nerves is avoided. My head-stall, though simple, would occupy much space to describe, further than to say that both chin and occiput are included in the mechanism.

The procedure is thus carried out. When all is ready, the apparatus found to fit comfortably, and assistants duly instructed, a leaden wire is laid on the spine, and an accurate pattern of its curve is taken. The height of the figure is also measured. Then the patient is suspended, and a flannel bandage rolled upon the figure from about the level of the trochanters to the armpits. A small pad should be placed over each anterior spine of the ilium, and if we are dealing with a young woman a layer or two of wadding should be placed

between the mammæ and also on the outer side of the glands. I also fold a napkin three or four times, and place it over the central parts of the abdomen. Over all these a prepared plaster-of-Paris bandage is applied, more plaster is rubbed in, and the whole allowed to become very nearly hard. The patient is then taken horizontally out of the sling and laid on a couch, the pads over the crista ilii and the napkin over the abdomen are withdrawn, and thus a space is left over each prominence so as to avoid pressure, and also, over the abdomen, room for respiration. In a few minutes more the plaster is quite hard, the patient may be placed on his feet, and the height of the figure is again measured. In my experience, it increases from three-quarters of an inch to two inches. The wire should be traced on paper, and the pattern dated and put aside for future comparison.

The patient, if the bandage have been skilfully applied, feels much more comfortable, and is able to stand, even to walk, with much greater ease than before. He should, indeed, be allowed, even encouraged, to get about. The bandage will continue on the figure for a time, which depends partly on the care taken to avoid chafing, partly on the docility of the patient, partly on the quality of the plaster; but if any smarting or other symptoms of abrasion occur at spots away from the edge, and out of reach of any interposed pad, it is better to remove the whole thing and reapply it with additional precautions. The second application can generally be borne longer than the first, about ten or twelve weeks. During this time, as absence of pressure is secured, cure by anchylosis goes on by rapid strides, and the more rapid because the patient's health is so much better than it can be when he is condemned to a bed or couch. There is also in this plan the additional advantage that the spine becomes fixed in as straight a posture as possible, instead of, as in the treatment by recumbency, in as crooked a posture as can well be attained.—*Lancet*, *June* 9, 1877, *p*. 829.

33.—UNUNITED AND ADHERENT TENDO ACHILLIS,

SUCCESSFULLY TREATED BY PARING THE ENDS AND STITCHING THEM TOGETHER WITH CATGUT SUTURES.

By Thomas Annandale, Esq., F.R.S.E., Surgeon to the Edinburgh Infirmary and Lecturer on Clinical Surgery.

The treatment of cases of ununited tendo Achillis, the result of an external wound, has hitherto been considered very unsatisfactory, more particularly when the divided ends have become much retracted and adherent to the skin and surrounding textures. The retracted ends of the tendon in such accidents have occasionally been brought together and secured by silver-

wire or other sutures; but the inflammation and suppuration which usually follow this proceeding have, in the majority of cases, rendered the operation a failure. Now, however, that we have, in the antiseptic treatment and catgut suture, sure means of making the necessary wound and securing the divided parts without risk of suppuration, I am encouraged to hope that the condition of ununited tendons will, in the future, prove more amenable and satisfactory in regard to its treatment. In confirmation of this hope I report the following case, which thoroughly proves the success of the antiseptic treatment in a very aggravated example of the accident under consideration. From the condition of the divided ends of the tendon and surrounding tissues, as accurately ascertained during the operation, I am of opinion that nothing short of the treatment adopted, or of some similar interference, would have restored the tendon to its proper state and usefulness.

J. P., aged thirty-seven, a strong healthy sailor, was admitted into my wards in the Royal Infirmary on the 10th March, 1877. Rather more than two months before admission the patient accidentally struck the back of his left leg with an axe and inflicted a wound which cut across the tendo Achillis a little above the ankle. After the injury the leg was kept in the straight position until the wound healed: but when the patient commenced to move about he found that he had little or no control over the foot. Finding that he was not improving, and being quite unfit to follow his employment, he applied for advice to Dr. Wilson, of Greenock, who sent him here to be under my care.

An examination of his condition showed a cicatrix across the lower end of the tendo Achillis, which had been completely divided. There was a distinct interval of fully one inch and a half between the divided ends, and there was not the slightest attempt at any union between them. The upper end was adherent to the skin, and when the muscles of the calf were put into action the tendon only drew upon the skin and surrounding tissues, and had no direct influence upon the heel. In consequence of this condition the patient's foot was useless, and he was quite unable to follow his employment.

On the 13th of March I performed the following operation, with the hope of relieving his unfortunate state. The leg, as far as the knee, was rendered bloodless according to Esmarch's plan, and then an incision about three inches long was made on the inner margin of the tendon, so as to expose its ends where divided. A little careful dissection thoroughly disclosed the affected parts, when it was found that the tendon had been completely divided about one inch and three-quarters from its insertion into the os calcis. The divided ends were retracted for

about an inch and a half, and between them was a thin-walled cyst or sac containing a little fluid serum tinged with blood. There was no trace of any new organised material forming a bond of union between the divided ends, but the lower end was rounded off in the most perfect manner. The upper end was somewhat enlarged and jagged in appearance, and was adherent to the skin and cicatrix of the external wound. Having first freed the upper end from its attachment to the skin and cicatrix, I pared both ends of the tendon, removing a thicker slice from the lower than from the upper one, on account of the rounding off of the former. Then, by flexing the leg to almost a right angle, the ends were brought in contact and secured by means of two prepared catgut sutures of double "medium" thickness. The limb was then firmly adjusted in this flexed position by applying the apparatus usually employed in the treatment of ruptured tendo Achillis. The operation was performed under the antiseptic spray, and the wound was dressed in the usual way. The antiseptic dressing was continued, and changed as often as required, until April 1st, when, the wound being quite superficial, boracic lotion was substituted for it. Three weeks after the operation the parts were carefully examined, and, as good union had taken place between the ends of the tendon, the limb was slightly straightened, so as gradually to stretch the new material and obtain the proper lengthening of the tendon. This treatment was carefully continued, and on April 22nd it was found that the result of the operation was perfect. The patient had entire control over the foot, the union of the tendon was strong and complete, and the heel could be brought to the ground without any difficulty.—*Lancet, May* 5, 1877, *p*. 638.

34.—SURGEON-MAJOR PORTER'S SAWDUST PADS.

By George W. Callender, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

I will first say how the pads are made; secondly, relate the cases in which they were used; and thirdly, express my opinion as to their value in surgical practice.

The sawdust is obtained by preference from the Memel pine; that from red deal may also be used, either of these containing a large amount of terebine. The dust from hard wood will not answer, as Mr. Porter finds that it does not absorb freely. It has first of all to be well sifted, for, as supplied from the works, it often contains coarse fragments which would cause, under pressure, hurt or inconvenience. The fine sawdust is then enclosed in muslin of such quality as will just prevent its escape. The bag, when made, is shaped for each case as may

be required; when about three-fourths full it is closed, and is then quilted, otherwise the wood-dust will gravitate, or under pressure will be displaced entirely from certain parts of the bag. As to the muslin I have ventured to depart from Mr. Porter's practice in using ordinary instead of antiseptic gauze, no advantage being gained by the use of the latter. The pads thus made are applied either to side splints, or to cover an ordinary back splint (as for a compound fracture of the leg), or over abscess wounds, or over suppurating surfaces, or over dying or dead tissues; they are used, in fact, either as pads or as the dressing over any part.

The following, amongst others, are cases in which they were employed:—

On March 31st, a male, aged forty-one, was knocked down by a locomotive on the Chatham and Dover line, and, besides lesser hurts, sustained a fracture of the pelvis, of several ribs on the left side, and had also about one-fourth of the scalp torn off from the right side of the vertex of the skull, exposing the bone. The wound was dressed with carbolised lint, and was then covered with a sawdust bag. No cerebral symptoms ensued, but there was bleeding on two occasions from the scalp, easily restrained by pressure. The blood was absorbed by the saw-dust, so that the bed-linen was not soiled. The patient slowly convalesced, and on the 16th of May was able to get up.

A male, aged thirty-six, was admitted on March 9th with a large abscess connected with carious disease of the bones forming the left shoulder-joint. The abscess was freely opened and washed out with carbolised water. An india-rubber drainage-tube was then introduced, and the wound was dressed with a large pad of sawdust fitted to the shape of the shoulder. At the time of the operation the joint was freely opened, and the carious bone was gouged and scraped away. The serum which oozed from the drainage-tube was entirely absorbed by the dressing. There was no offensive odour, and the pad was not changed for seven days.

On April 26th, a male, aged sixty-seven, fell off the kerb, and was run over by a heavy van. He thus sustained a compound fracture of both bones of the right leg, the soft parts being severely damaged by the pressure of the wheels passing over them. The limb was supported on sawdust pads fitted to an ordinary back-splint, and the wound was covered with lint soaked in carbolised oil. When I saw the patient, it was evident that very extensive sloughing of the soft parts must ensue. The whole leg was therefore enveloped in lint soaked in carbolised oil and covered with gutta-percha tissue. Thus, and with the pads, which were saturated with discharge and required changing about every four or five days, the pro-

cess of sloughing was passed through without any unpleasant odour and without constitutional disturbance. The utility of the pads in this severe case was marked, the discharge being freely absorbed by them, and remaining inodorous. The patient is now—three weeks after the accident—well in himself; he has, however, to heal up an extensive surface left in a state of ulceration by separation of the sloughs, and it is more than doubtful if he will have the strength to do so.

On May 1st, a male, aged twenty-four, was caught between a fly-wheel in motion and an adjacent wall, so that his left leg was crushed, the bones being fractured in many places, and the soft parts being extensively lacerated. I found it necessary to perform a primary amputation, as low in the thigh as the bruised condition of the soft parts would permit. The stump was drained, and was dressed with carbolised oil on lint and gutta-percha tissue over this. The limb was then swung on a drop-splint, resting on sawdust pads. There was a great deal of blood-stained serum, and afterwards of grumous fluid, which oozed from the bruised tissues of the stump; but the parts of the stump in apposition healed by the first intention. The discharge abovementioned was received on the drop-pad, which was removed daily. The pad which bore the weight of the limb, and the limb itself, were not disturbed for fourteen days. The pad when removed was perfectly sweet, and no serum had run into or in any way soiled the bed-linen. The patient is now—May 22nd, three weeks after the operation—convalescent, having recovered without an unfavourable symptom, only his recovery was slow, owing to the quantity of blood lost whilst being brought up from Blackwall to the hospital. In this instance the pad was most serviceable. It enabled us to leave the thigh undisturbed for the time mentioned; and, from its yielding, it allowed the limb to mould for itself its bearings, which were throughout free from all discomfort. With the drop apparatus we were able to dress the stump without causing the patient the slightest pain.

A girl, aged 16, required to have her leg amputated immediately above the ankle-joint (Syme's operation) on account of carious disease of the left tarsus. The stump was drained, covered with carbolised oil on lint and gutta-percha tissue, and was secured on a drop-splint and swung. The small sawdust pad was changed during the first week every second or third day; the larger pad which supported the leg was not disturbed in any way for three weeks; when removed it was sweet and clean.

A woman, aged seventy-nine, was knocked down and run over by a heavy cart. She sustained a compound and comminuted fracture of the bones of the right leg, and a com-

minuted fracture of those of the left. The compound fracture was put up on a back splint, with sawdust pads, and was then swung. The wound was cleansed with carbolised water, and was dressed with lint soaked in carbolised oil. The sawdust pads were renewed seventeen days after their first application, but the dressing of the wound has not been disturbed, and the patient, over three weeks after the accident, is now convalescent.

A strumous lad, aged fourteen, was taken into the hospital with acute necrosis of the shaft of the tibia, involving also the lower epiphysis, and attended with destructive inflammation of the ankle-joint. The suppuration was profuse. The leg was swung in a sawdust bag, with great comfort to the patient, especially as it is now found to be necessary to change the supporting pad only once every ten days, instead of daily, as before its use. The relief to the patient is shown in the improvement of his health, he having increased ten pounds in weight since the greater ease and quiet thus gained for him during the last six weeks.

Apart from the question under consideration, these cases are of interest with reference to the results obtained in the treatment of severe wounds, and extensive suppurations. As to the use of the pads, it may be said that they are approved by the sisters for their cleanliness, and for the manner in which they keep the bed-linen from being soiled by discharge of serum or of pus. They are easily made so as to fit as required, and they are inexpensive. When the quilting is properly attended to they are comfortable to the patient, readily yielding to such pressure as that, for instance, caused by the weight of the leg, and moulding so as to give equable support. Whilst they effectually absorb discharge, it is as well, when this is considerable, that the pad should be changed every two or three days, but when, in addition to the pad, carbolised-oil dressing is used, they can be left for a longer period. Thus, in the case of the two amputations, the pads which supported the leg in one, and the thigh in the second, were not touched for three weeks, and for fourteen days, respectively. I do not feel disposed to rely entirely upon these pads for keeping parts absolutely clean; but, in conjunction with carbolised oil, or with some kindred dressing, they are amongst the best pads with which I am acquainted, and I consider that we are much indebted to Mr. Porter for giving us an appliance which is simple, inexpensive, and efficacious. I may add, that, mixed with shot, so as to give weight to the appliance, these pads may be used to make pressure, when such is desirable, as over some forms of abscess, to prevent redistension from collection of pus in a sac which has been opened.—*Lancet, June* 23, 1877, *p.* 902.

35.—ON SUBCUTANEOUS OSTEOTOMY IN RACHITIC AND OTHER DEFORMITIES.

By S. Messenger Bradley, Esq., F.R.C.S., Surgeon to the Manchester Royal Infirmary.

Our experience in Manchester is bright, for during the last year Mr. Lund has performed osteotomy of the neck of the femur three times; Mr. T. Jones, at the Children's Hospital, has subcutaneously divided the tibia eight times, and I have performed a similar operation four times, all cases terminating favourably. Perhaps these few statistics may serve to show you that, with certain precautions shortly to be mentioned, the operation is not one dangerous to life, while the results are so uniformly excellent that it may fairly be held to have established its claims to be considered one of the standard operations of surgery.

Let me briefly draw your attention to some of the cases in which we have found it necessary to perform this operation in Manchester. This little girl, six years of age, was admitted into the infirmary on October 13th, 1876, with extreme rachitic deformity of both legs. The convexity of the curve on both sides was outwards, and confined to the lower third of the leg-bones. Like most cases of rachitic deformity, it manifested itself whilst the child was learning to walk, and through the mother's carelessness it reached the condition presented on admission, when locomotion to any extent was no longer practicable, the outer side of the ankle and the leg above it touching the ground when the child tried to stand upright. There was also some anterior bowing of the tibiæ. Having proved, under chloroform, the impossibility of forcibly straightening the limbs, I proceeded on October 20th, 1876, to divide the tibia on one side with hammer and chisel. I was induced to try this plan, so strongly advocated by Maunder, and so extensively adopted both in London and on the Continent, on account of the objections which were originally raised to Langenbeck's operation with augur and saw, and which has since been renewed, that the resultant bone-dust left by the saw rendered the parts prone to suppuration. I accordingly made an incision three-quarters of an inch long on the inner concave aspect of the tibia and down to the bone, and then proceeded to divide it with the hammer and chisel. The bone, however, proved so extremely hard—sclerosed, in fact—that I desisted from further use of the chisel, and completed the division (very easily) by means of a narrow-bladed saw. On completing the division of the tibia, the fibula was so hard I could not break it, and I was consequently compelled to enlarge my original incision by a cross cut, and to saw the splint bone

separately. The operation was performed under the carbolic spray, and the usual dressings applied. No particular fever followed the operation, and on the fourth day I restraightened the leg, and put it on a back splint with an outer side splint. Throughout the dressings were changed under the spray. Some suppuration ensued, and it was not until December 10th, 1876, that the external wound had completely healed. At this time the bones were once more firmly united, and, as you see, the limb was perfectly straight. Now, note the different progress of the case when the bones of the other leg were divided. On November 17th, 1876, I proceeded to divide the tibia on the opposite side. This time I did not use the hammer and chisel, but employed a tenotomy knife and a short straight narrow-bladed saw. The division was effected with the greatest possible ease through a small puncture made with the tenotome, and when the division was complete, and the leg brought into a straight line, the punctured wound was closed with collodion, and the limb put up on a back splint. On this occasion the spray was dispensed with. On the third day I found the external wound firmly closed up, and all that we had to wait for was the consolidation of the divided bones. This proceeded satisfactorily, and, as you see, the limb is now quite straight and strong. In a word, it required six weeks to heal the wound made with the chisel, and two days to heal that made with the tenotome and saw; so that it certainly seemed to me that we had in these two operations a fair example of how to do it, and of how not to do it. At the present moment, as you see, the legs are straight, and the bones quite strong, but walking is still very imperfectly performed. This is due partly to wasting of the leg muscles and partly to want of practice. For the time, locomotion is a forgotten art, and will have to be learnt over again. One other point merits attention: observe how loose the knee-joint is. So loose, indeed, is the entire articulation that you can place the leg laterally at almost a right angle with the thigh. This is a common feature in rickets, and one which constitutes a special difficulty in treatment. The epiphyses are large, and the ligaments so loose that the limb is almost like a flail. The only method of overcoming this is to maintain the limbs in a straight line, by means of starch bandages or some other immovable apparatus, and at the same time, by appropriate general treatment, sea air, exercise, lime, &c., to endeavour to remove the diathetic taint.

The sturdy little boy who stands by the young lady's side is another example of the same disease and of the same treatment. His legs, which now are straight enough for anyone, were as curved as a Turkish scimitar when he came into the infirmary on October 13th, 1876. He was then only three years old, but

already the bones were so firm that I failed to straighten them by forcible handling under chloroform. I consequently divided both tibiæ a few days later, on their inner or concave aspects, by means of the tenotome and narrow saw, easily fracturing the fibulæ on both sides. The punctures were sealed with collodion, and the limbs put up quite straight on a back splint. From these back splints they were not removed, nor were the wounds redressed, for three weeks, when firm union was found to have taken place. In this case the spray was not employed. I cannot say precisely when the external wounds healed, as the bits of lint were not removed, but this doubtless took place in a day or two; there certainly was no suppuration or inflammation. Besides these two cases of union, Mr. T. Jones has had other four cases at the Children's Hospital of a similar nature, dividing eight tibiæ with tenotome and saw, all turning out perfectly satisfactory.

And now let me draw attention to another class of cases in which you have seen subcutaneous osteotomy performed for the relief of deformities. I refer to Mr. Lund's cases, where he has divided the femoral neck on account of bony anchylosis having taken place at the hip-joint, his object being to establish a false joint, and so to loosen the patient from his bony bonds and give him freedom. Mr. Lund first performed this operation on March 5th, 1875, on a man with rigid anchylosis of both hips, who was terribly incommoded by his condition, and unable either to walk or sit. The case has been already detailed in one of the medical journals, and therefore I shall content myself with brief reference. The operation was performed with antiseptic precautions, after Mr. W. Adams's plan, by means of a narrow-bladed knife and narrow saw. The man never had a bad symptom. Passive movement was commenced twenty-four days after operation, extension being meanwhile employed to keep the divided surfaces asunder. In eighty-four days, finding the patient able to support himself upon this leg, Mr. Lund proceeded to divide the opposite femoral neck in the same way. In fifty-seven days after the second operation the patient was permitted to sit up, and soon after he began to walk about a little. Since then he has gone on uninterruptedly well, and Mr. Lund informs me that quite recently he has heard from the man that he had walked a mile the day before. Mr. Lund has since performed the operation on three other cases, and with uniformly good results. In no instance has suppuration or pyrexia followed the operation, and in each instance he has been able to establish and maintain free and easy motion at the new joint.

Such cases as these serve to support the principle that subcutaneous operations are free from risk. You here see many

different structures, skin, fasciæ, muscles, vessels, nerves, and bones, divided without any disturbance of the general economy, and we are in a position indeed to affirm that subcutaneous surgery ought never to be followed by putrefactive inflammation. I do not say by inflammation, for this may happen in spite of every precaution, and, indeed, does happen. We often meet with cases of cellulitis, &c., without any lesion of skin, and not unfrequently we find that simple fractures, which are *de facto* subcutaneous wounds, are followed by cellulitis; but we also invariably find that when such is the case the inflammation runs a simple course, the products either being absorbed, or, if they pass into a state of suppuration, that the resulting abscess is limited by a distinct wall, and never leads of itself to septic change.

Perhaps the experiments of M. Guérin in 1840 demonstrate with more force than others the absolute safety of subcutaneous wounds. Amongst many other experiments on an equally liberal scale, he, for example, subcutaneously divided in a dog all the spinal muscles from neck to sacrum, and in the same victim, all the muscles, vessels, and nerves on the outer side of both thighs, and all this butchery was done without constitutional disturbance, and with rapid and completely non-inflammatory healing. The exclusion of air, then, secures a good result, and renders an operation safe; but we now know that it is not the air which does harm, but something in the air, and that if we can exclude this something we may allow the air free entrance. The researches of Pasteur, corroborated by the experiments of Tyndall, Lister, and Roberts, lead us to the conclusion that the cause of all putrefaction—and it is putrefaction which we have to dread—is the presence of bacteria, and hence to exclude bacteria, or what comes to the same thing, to destroy the bacteria already present, is to render putrefaction impossible.

Of course you will readily understand that all air is not equally impregnated with these organisms: thus, *e.g.*, Tyndall recently found that his experiments as to the causes of putrefaction were rendered very difficult, and even nugatory, when performed in the vitiated air of the Royal Institution, but that he was able to perform the same experiments with ease and success in the purer air of Kew; and for the same reason it follows that we may securely perform operations in some atmospheres without those precautions which are absolutely necessary in an air less pure.

The object of the antiseptic method is in the main to prevent putrefaction, by excluding or destroying these mischief-making germs; hence the *rationale* of subcutaneous surgery and antisepticity in surgery is one and the same—viz., not to exclude

the air, but to shut out bacteria. Neither plan prevents the advent of inflammation, but both hinder putrefaction and consequent septic evils. Thus, for example, you learnt in the first case, which I showed you to-day, that considerable suppuration had followed the operation, but that, owing to antiseptic treatment, the pus had no chance of putrefying and poisoning the system. At the present time I am engaged in testing the germicidal properties of various agents, such as carbolic acid, salicylic acid, boracic acid, iodine, bromine, chloride of zinc; and though in doing this I am only going over ground which has been in a great measure traversed before, and although I do not even hope to see results more regular or more satisfactory than are already secured by the method of dressing introduced by Lister, I do not, therefore, deem it a necessarily unprofitable labour, but one which may prove of some practical value, if it be only to simplify the at present somewhat complicated dressings and troublesome details of the antiseptic method. In regard to the use of the spray and of the special form of dressings adopted by Lister, I must confess, indeed, to a feeling of doubt as to their ultimate retention by the surgical profession. In a very foul atmosphere, where bacteria abound, it may be well to employ some such means as the spray-producer of keeping the parts free from falling germs, but in a pure air this can hardly be necessary. And even in our much vilified infirmary atmosphere we may fairly doubt the need of this troublesome impedimentum; for, as a fact, bacteria do not here abound, but, on the contrary, as Dr. Dreschfeld and I have both found, are very few and far between; certainly they do not hover like a cloud above the hands of the operator. Nor is this all that is to be said in criticism of the use of the spray, but we may also question its efficacy as a germicide. I do not think the carbolic vapour produced represents a higher percentage of the acid than 30 per cent., and we have reason to greatly doubt whether this strength is fatal to the life of bacteria; so that on the whole I fail to see why the same object would not be equally well and certainly attained by previously washing the parts (as is already done, indeed) with carbolic soap, and by taking the greatest care that every instrument and sponge is perfectly clean. It is quite certain that if it turn out that we may safely dispense with the spray and the special dressings of Lister, the antiseptic method will have many more followers than at present.

Turning, however, from this criticism of some of the details of antiseptic surgery to the broader view of its real value, we may confidently affirm that, antisepticity being strictly enforced, it matters little whether we employ the hammer and chisel of Maunder or the saw and tenotome of Adams, as in both cases

we can as confidently predict the result as when dealing with a wound altogether subcutaneous; but, neglecting such precautions, it will probably make all the difference in the world whether we have to deal with the large open wound of the one operation or the small punctured wound of the other; and, further, it may be added that although the risk of septicæmia is manifestly greater in the former than in the latter operation, antiseptic precautions being disregarded, he cannot be held blameless who, in the present state of knowledge, does fail to take these precautions in either one class of cases or the other.

Returning, however, to what is now strictly the subject in hand, I would not have you suppose, gentlemen, from what I have said, that every case of rickety deformity of the legs, or that every case of bony anchylosis of the hip, is to be treated by subcutaneous osteotomy; for, in coming to such a conclusion, you would greatly err. In the former condition, many cases are curable by less severe measures; in the latter, many cases are beyond the reach of cure even by such an extreme step, and therefore I perhaps cannot better conclude this lecture than by briefly indicating the different modes of treatment necessary in different degrees and kinds of the two affections. And first as to rickets.

The cases of rickets demanding surgical interference may be divided into three groups:—

1. *Cases taken at an early stage, when the crooked limbs yield to splints and bandages.* This is a large class, and the plan should always be tried to begin with, when the bone is at all soft and the child young. I am in the habit of advising the use of a light wooden splint, to be worn only at night, and applied along the inner side of the leg and thigh by means of a domette flannel bandage. I learnt this from Mr. Lund many years ago, and have ever since used it, and with benefit. If the case be a little too obstinate for this treatment, splints worn during the day as well must be substituted for those worn only at night.

2. *Cases incurable by the former method, but which yield to forcible manipulation under Chloroform.*—When rickety legs cannot be straightened by the use of splints, the child should be placed under the influence of chloroform, and an attempt should be made to rectify the deformity by forcible bending of the bones. Many cases yield to such treatment, and, in thus dealing with them, it matters very little whether in our efforts to straighten the bone we break it (osteoclasm) or not, as the fracture so produced always unites, and the leg is not one bit the worse or the weaker in the long run.

3. *Cases which have resisted both the former methods of treatment, but which may be cured by Subcutaneous Osteotomy.*—This is the class to which I have specially drawn your attention this

morning, and which, I think, may be said to include all cases not coming under the first two heads. From time to time surgeons have advocated the use of instruments, in appearance worthy of an old torture-room, to break the crooked rickety bones when they resisted the pressure of the hands; but by most these appliances are relegated to the limbo of forgotten rubbish, and, as it seems to me, wisely, since we have in subcutaneous osteotomy an operation at once so safe and so satisfactory. And one final word on this subject. When subcutaneous osteotomy is performed for the cure of rickety legs, we need be under no apprehension about the filling up of the V-shaped interval which is necessarily left when the bones are straightened, for, as a matter of experience, this gap always does fill up with fresh bone, and thus we secure not only a strong and straight leg, but a limb not shortened, as it would of course be if we were compelled to excise a wedge-shaped piece of bone instead of simply sawing across the concavity.

Now, turning to bony anchylosis of the hip, let us tabulate the various kinds of this condition and the different treatment necessary, as we have already done with rickety deformities. In doing so, I shall in a great measure follow the classification of Mr. W. Adams. The anchylosis referred to is always bony.

1. Cases of anchylosis following injury in the healthy subject, and also cases of anchylosis following the long retention of one position, also in the healthy subject. In both, subcutaneous osteotomy is the proper treatment. I have already given my reasons for preferring the use of the tenotome and saw to the hammer and chisel.

2. Cases of anchylosis due to rheumatic inflammation, and cases of anchylosis due to chronic pyæmic inflammation. In both, subcutaneous osteotomy *may* be performed. In both the first and second group of cases the operation is performed either with a view to rectify an abnormal position of limb and to reproduce an anchylosis in the straight line (Adams's operation), or it is undertaken when the limb is already in a right line with the trunk, with the intention of procuring motion by the establishment of a false joint (Lund's operation). It is manifest that if the latter object be aimed at, success will more likely follow in the traumatic and pyæmic cases than in those due to rheumatic mischief, as the proclivity to a restiffening of the new joint and the recurrence of anchylosis is so much greater in the latter.

3. Cases of anchylosis in strumous subjects, following strumous arthritis, and followed by partial or complete absorption of the head and neck of the femur. In none of these cases is Mr. Lund's operation indicated, but in the milder set of such cases Mr. Adams's method may be adopted with reasonable hope of a

good result. When the neck is absorbed, of course it cannot be divided, and therefore Adams's operation cannot be performed; but even in such extreme cases, if the position be very faulty, the plan recommended by Mr. Galt, of dividing the femur by means of a fine saw immediately below the lesser trochanter, may be carried out with a fair prospect of success.—*Lancet, July* 21, 1877, *p.* 78.

36.—PSOAS ABSCESS—HYPERDISTENSION WITH CARBOLISED WATER.

By Edmund Owen, Esq., M.B., F.R.C.S., Assistant-Surgeon, St. Mary's Hospital, London.

[In consequence of reading Mr. Callender's paper on treatment of abscesses by hyperdistension with carbolised water, Mr. Owen tried the plan in the following case :—]

On Saturday, 9th of September, a pale, sickly-looking boy, with patches of deepish ulceration on his cheeks, was admitted to the children's ward under my care. He was between six and seven years of age, and was carried into the hospital by his sister, who was scarcely bigger than himself. He was in a most pitiable condition, with an enormous abscess on the upper and outer part of the front of the right thigh. The skin in that neighbourhood was red and thin, and it was evident that unless something were done for him without much delay, the wall of the abscess would give way certainly if it met with any rough usage. There was angular curvature of the spine at the upper lumbar region, and great fulness extended up the flank under Poupart's ligament.

Up to within a short time he had been able to play about with his companions, but now he was in such an exhausted state that it would have been unsafe to have sent him away. He was therefore placed in bed, and on the following Wednesday, he having been placed under the influence of chloroform, an incision was made in the most prominent part of the swelling, and of sufficient size to admit one's finger. From this opening a large quantity of thick and healthy-looking pus escaped, and when the flow was becoming slow the nozzle of a large syringe was introduced, and the cavity completely washed out with a warm solution of carbolic acid and water. The edges of the wound also having been held securely over the syringe, the cavity of the abscess was more than fully distended with the injection, which was allowed to be retained "while one with moderate haste might tell a hundred." A drainage tube having been introduced well up into the loins, and the wound covered up with oiled lint, the boy was sent back to bed. When I went to see him an hour afterwards, he was half sitting up in bed

playing with his toys, and talking to other children in the ward.

Next morning his pulse had risen to 108 beats a minute, and his temperature to close on to 100°. Mr. Coumbe, the house-surgeon, who has syringed out the cavity every other day, tells me that the boy's temperature has never reached 101°; and that he has been in no pain, but that he has eaten, slept, and drunk well. The drainage-tube does not pass as far up as it did at first, but it is generally kept in the wound. The discharge has continued sanious since the second dressing.

I may here state that the boy is still in the hospital, but I intend shortly to case his trunk in a large and stiff plastic splint, and to have him removed to his home.

The other case is that of a boy of seven years, named Ambrose, who was brought up from the country into my out-patient room at the Hospital for Sick Children on the 20th of last September.

From notes which were taken by Mr. Cant, the house-surgeon, it seems that he has five well-conditioned brothers and sisters, and that his father and mother are alive and well.

He himself had always been a healthy boy until a year ago, when some of his young friends had been amusing themselves by jumping on his back. Shortly after this he complained of pain, and walked lame. Two months since a swelling appeared on the front of his thigh, and this, at the time of his admission as an out-patient, was very extensive and pointing. The skin was red, thin, and very tense. There was well-marked angular curvature in the lower dorsal and upper lumbar region. It was evident, or at any rate highly probable, that if he were sent home the integument might give way on the journey.

His temperature was about normal, and his pulse 105. The tongue was coated, and the bowels were confined.

He was taken at once into the operating theatre and treated like the patient whose case I related just now, except that no oiled lint was applied over the wound.

Next day his temperature had risen to 101°, but gradually coming down, it remained at about normal for twelve days.

Every other day the sac was washed out; the boy remaining in very good health. At this juncture the drainage-tube slipped out, but was replaced, as nearly as the sister of the ward can remember, six days afterwards. But in the meanwhile his temperature had risen to 103°. For a few days it varied from 100° in the morning to 103° in the evening. At the present time the morning temperature is but little above the normal. The boy is dreadfully excitable, but eats and sleeps well. He still complains at times of pain in the back, but since he has been in the hospital his physical condition has greatly improved.

Remarks.—The great feature in this method of treatment of

psoas abscess is its simplicity, and the absence of all special and intricate dressings. All that are required being a scalpel, the warm solution of carbolic acid, of the strength of one part in thirty, an ordinary enema syringe, a long probe, and a drainage tube. The best dressing consists in fine picked oakum, for soaking up the subsequent discharges, and a bandage.

Of course one cannot attempt to predict the unfailing good results for all psoas abscesses treated in this way. On the contrary, I am strongly of opinion that nothing is more perversive of the truth in surgical practice than may be first successful cases. Still from what I have heard of this method of treatment of psoas abscess in Mr. Callender's hands, together with my own limited experience, I feel sure that we are advancing towards a happy solution of a most difficult question. Hitherto surgeons have, for the most part, dreaded interference with these abscesses of the spine. They have deferred active treatment of the abscess as long as possible. Nature has, indeed, often been allowed to step in and effect an outlet in her own way. Hectic, profuse discharges and death have then closed the scene. So-called valvular incisions, partial emptyings, and aspirations have been attended with no better result.

However carefully punctures or incisions may have been made, with a view to prevent communications between the cavity and the exterior being permanent, I have always found that the pus has taken advantage of them, and that a constant drain has followed shortly after.

Professor Lister has done much towards establishing a correct method of the treatment of chronic abscess. He has shown us what free incisions for the outlet of pus, the use of drainage tubes, and a due attention to hygienic matters can effect. The complication of antiseptic gauzes, many folds of "protective," and whatnot, have, however, placed his method of treatment beyond the reach of the single-handed and busy surgeon. Indeed, few dare call themselves thorough practitioners of his school, unless they have had the advantage of studying under his able guidance, and following him daily through his wards.

The simple treatment of psoas abscess which I have described, and which has been called the treatment by hyperdistension, seems to be antiseptic treatment carried to the furthermost limits of the disease; and a great part of its perfection consists in its absolute simplicity.

I do not imagine for a moment that my two cases of caries of the spine are cured, but I think I may say that they have been safely guided through a much-to-be-dreaded and critical stage of their disease.—*Medical Press and Circular, Nov.* 15, 1876, *p*. 393.

ORGANS OF CIRCULATION.

37.—THE CATGUT LIGATURE.

There is no subject of greater importance to surgeons than the safety and utility of the catgut ligature, and the cases presented to the Clinical Society by Mr. Bryant are a valuable addition to our knowledge. We wish here to draw attention to his conclusions, viz., that a catgut ligature, like a permanent ligature, *divides at once the two inner coats* of an artery; that at a later period it more or less completely severs the outer coat; that it is the best ligature, as the outer coat has not to be *completely* ulcerated through to allow the escape of the thread. Are not these conclusions beside the real point at issue? Surely no one has ever doubted that catgut, like any other thread, *can* divide the two inner coats of an artery, or that the outer coat will be severed if it be strangled, whether by catgut or silk. Used in this way it is attended with all the risks of a silk ligature, and in addition some peculiar to itself arising from its power of softening in living tissues. What is wanted is a mode of permanently closing an artery without exposing it to the risks attending injury or ulceration of its coats, and this has been sought in the formation of a ring of living tissue encircling the vessel. Mr. Lister has recommended catgut to be applied so as to close, not injure, the artery. Then as a result of the wound down to the vessel, and also from the presence of the ligature, lymph is effused which organises, contracts, and around the ligatured artery forms a firm ring of cicatricial tissue, which permanently occludes the vessel. What is needed is something which will resist the pressure of the blood until the lymph has, by its organisation, acquired sufficient strength, and then disappear. It was hoped that catgut would fulfil these conditions. It has been proved to disappear when embedded in living tissues, and Mr. W. J. Fleming has described the changes seen in it—its softening and infiltration with cells, the development of vessels, and further organisation of the new tissue. The point of greatest interest is the rapidity with which these changes occur. From the time of the application of the ligature the anastomotic circulation develops, and the pressure of the blood against the barrier to its progress diminishes. Will the catgut remain firm until this pressure has so diminished that the lymph around the artery is able to resist it? If not, the constricted vessel opens out again, the blood pursues its old course through an *uninjured* vessel; the operation will have failed, but will not have done harm. It must always be remembered that Mr. Lister founded his statements and recommendation of catgut on the results of experiments in

which his antiseptic treatment was carried out with precision, and he attached very great importance to the perfect prevention of putrefaction in and about the wound. Is it right to test his statements by other experiments in which his conditions are not observed? What we need is a series of cases in which the catgut was applied to arteries so as *not* to sever any of their coats, and where the antiseptic treatment was successfully employed. It will then, and only then, be time to try whether we can arrive at the same results under different conditions.—*Lancet, Oct.* 20, 1877, *p.* 583.

38.—THE CATGUT LIGATURE.

By George Mowat, Esq., Swansea.

The following cases may probably be instructive, as showing the advantages of catgut ligature over silk in the treatment of varicocele.

On April 6th of the present year I performed the operation for cure of double varicocele by ligature. A straight needle threaded with silk was passed between the vas deferens and the veins, which was then made to re-enter where it had come out, care being taken to keep the point close to the scrotum until it reached the first puncture, it was then passed out, and the veins tied in the loop of silk, the ends were left long, and were twisted round a piece of quill, and tightened every day. The progress of this case was most tedious, the suppuration causing considerable annoyance. The first ligature separated on the twelfth day, but the second not until the twenty-first.

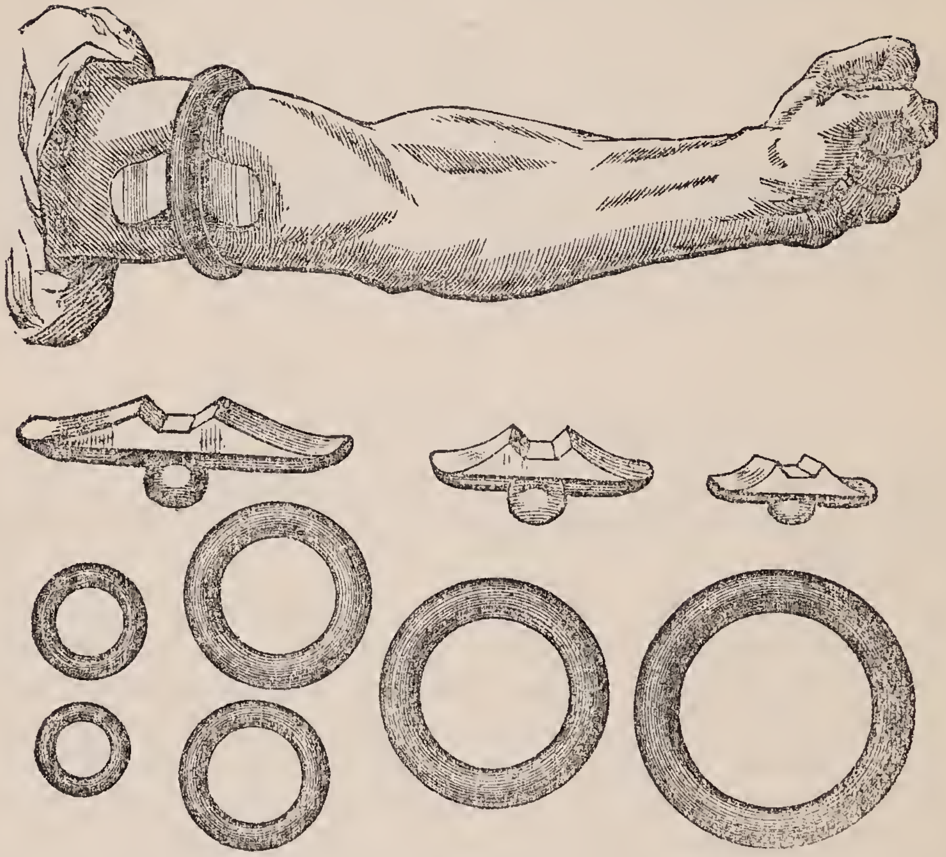
On the 19th of July I had occasion to perform a similar operation for varicocele of the left side, but determined to use carbolised catgut instead of silk or wire. The operation was done as I have just described, medium-sized catgut being used. No suppuration ensued. A firm mass of about the size of a nut was formed around the ligature. The man sat up on the third day, his testicles being supported by a suspender; and on the 30th of July he passed from under my care to all appearances cured.

A third case was operated upon on the 16th of this month in a similar way to the last. There was neither pain, suppuration, nor increase of temperature. The young man got up on the third day, and when his scrotum is suspended he can walk about his room with comfort.

All three operations were undertaken for the cure of nocturnal seminal emissions.—*Lancet, Oct.* 27, 1877, *p.* 633.

39.—BLOODLESS OPERATIONS.

Mr. H. L. Browne's New Appliance.



By this invention the blood is entirely compressed out of the limb, and is kept out as long as may be wished. (It is manufactured by Messrs. Salt and Son, of Birmingham.)—*Lancet, June* 3, 1876.

40.—THE RAPID CURE OF POPLITEAL ANEURISM BY ESMARCH'S BANDAGE.

By Thomas Smith, Esq., F.R.C.S., Surgeon to St. Bartholomew's Hospital.

[The following case is one of great interest. The treatment was carried out by Mr. Archer, Mr. Smith's house-surgeon, from the time the bandage was applied.]

R. F., aged forty-five, a bonnet-blocker from Luton, was admitted into Henry ward under Mr. Thomas Smith's care on March 7th, 1877, with an aneurism in the right popliteal space about the size of a hen's egg. The patient, a spare healthy man, noticed the swelling three weeks since. He says it came of itself, and when first observed was about the size of a walnut. It has gradually increased, and is increasing in dimensions pretty rapidly. The aneurism pulsates very forcibly; the circulation can easily be controlled by pressure on the femoral,

but flexion of the limb does not arrest the flow of blood through the sac. The patient complains of severe pain, especially at night, and in the recumbent position. Family history good; no personal history of syphilis; first sound of heart has a blowing character; urine healthy.

A few days' rest in bed diminished the tension of the aneurism and relieved the patient's pain considerably.

On March 17th, at 3 p.m., the treatment was commenced. The limb was rolled in a flannel bandage from the toes to the lower part of the popliteal space, and again from above the aneurism to the groin. Esmarch's india-rubber bandage was then applied, with only moderate firmness, from the toes to the aneurism, the patient being in bed; he was then made to stand up until the sac was well filled with blood, when the elastic bandage was applied from above the aneurism to the groin, where the limb was surrounded with the thick india-rubber tubing so as completely to arrest the circulation in the limb. The aneurism and popliteal space were thus left exposed, so that the least pulsation in the sac could be detected. In this way the circulation was stopped for one hour, during the last half of which chloroform was used on account of the pain. At the end of the hour, while the patient was still under chloroform, Esmarch's bandage was removed, and the Italian tourniquet was applied to the femoral and maintained in position for two hours. At the end of the first hour of the tourniquet pressure the patient began to complain of intense pain, and shivered a good deal, feeling, as he expressed it, "thoroughly cold." At 6.10 p.m., when three hours had elapsed, the tourniquet was removed on account of the intense pain, the patient refusing to take any more chloroform and being unable to bear the pain. The aneurism was found to be solid, and about half the size it had been at the commencement of the treatment. Towards night the patient complained of pain in the limb, and his temperature rose to 100·2°. Next morning he was still suffering from the effects of the chloroform, but had lost all pain in the limb, and in other respects was quite well. He was discharged from the hospital on April 6th, with the aneurism cured.

This case I believe to be of interest as adding to our knowledge of a method of treatment concerning which we need further experience before we can assign to it its proper function in the cure of aneurism. We are as yet ignorant of the precise details that may be advisable to adopt in carrying out this plan, and we do not yet know to what cases it may be best suited.

There can be no question that by Esmarch's bandage we can maintain a complete and continuous arrest of the circulation in a limb; an arrest that is not liable to interruption by the movements of the patient or the want of skill in his attendants—

contingencies that are inseparable from the employment of a tourniquet.

Esmarch's bandage may thus be regarded as the most perfect means we at present possess of carrying out in certain localities the principles of Dr. Murray's rapid treatment of aneurism by pressure. It has been employed successfully by Dr. Reid, Mr. Wagstaffe, Mr. F. A. Heath, Mr. T. Wright, and unsuccessfully by Mr. Bradley. It is likely that many others have applied this treatment to the cure of aneurisms, though, perhaps, with indifferent success, since it is probable that mostly the cures have been published. My own experience of this method of treatment consists of three attempts to cure popliteal aneurism, two being on the same patient, and on both occasions I was unsuccessful. The first failure, as I believe, was chiefly due to my having nearly emptied the aneurism of blood by pressure on the sac, and in the second attempt I could not be sure that the circulation in the sac was completely arrested, as I had covered the part with a bandage. In addition to these errors in the application of the treatment, I am now convinced that the arrest of the circulation was not maintained for a sufficient time to give the blood in the sac a fair chance of coagulating, even if other circumstances had been favourable to this process, which they were not.

In Mr. Bradley's unsuccessful case, he also made two attempts on the same patient to cure popliteal aneurism. His method of applying the treatment seems to have been essentially the same as was employed with success by myself and others.

As regards the plan pursued in the cases cured by Esmarch's bandage, the following is a short summary of the chief points of interest.

In all, the aneurism was of the popliteal artery. In all but one case—Mr. Wright's—the treatment was commenced by completely arresting the circulation in the limb for *one hour* by means of Esmarch's bandage, pressure being kept up after this time by means of a tourniquet. The tourniquet pressure was maintained in Reid's case for twelve hours; in Wagstaffe's for seven hours and a half; in Heath's case for five hours; in my own case for two hours; in Mr. T. Wright's case, Esmarch's bandage was kept on for two hours and a quarter, and at the end of this time pressure was kept up with more or less completeness for five days by means of a shot-bag. As to the time occupied in the cure, pulsation in the aneurism was known to have ceased at the end of fifty minutes in Mr. Reid's case; in two hours in Wagstaffe's case; in one hour in Heath's case; in three hours in my own; and in twenty-four hours in Wright's case. In this last case the pulsation returned to some extent after being absent for a few hours.

It is probable that coagulation of the blood in the sac may have occurred in these cases before the fact was ascertained by observing the absence of pulsation; in my own case, at the end of one hour from the commencement of the treatment, when the bandage was being changed for the tourniquet, it was observed that coagulation had not taken place, while at the end of two hours from this time the sac had ceased to pulsate; but during these two hours the condition of the blood in the aneurism could not be ascertained, as the pressure on the femoral was rigorously maintained. It was observed, however, that at the end of two hours from the commencement of the treatment the patient complained of very severe pain, and was attacked by shivering. At this time, I believe, the coagulation in the sac occurred.

With reference to the manner in which the pressure was applied, it is to be noticed that in Wagstaffe's and Heath's cases the elastic bandage was applied to the whole limb—with moderate firmness below the aneurism, loosely over the sac, and tightly above the sac; no elastic ligature was used at the groin. In Mr. Wright's case and my own, in addition to the elastic bandage to the whole limb, excepting over the sac, the elastic ligature was used below the groin. In Dr. Reid's case an elastic ligature was used below the groin, and no elastic bandage was employed to the limb. In Mr. Bradley's case and my own the aneurism was left quite exposed, so that its condition could be freely examined as regards pulsation and solidity. In three of the cases recorded, during the continuance of the treatment and probably at or soon after the occurrence of coagulation in the sac, a small artery was noticed to be pulsating vigorously over the aneurism. In all the successful cases the bandage was so applied that the sac was filled with blood at the time the circulation in the limb was arrested.

From the consideration of these cases, it seems that the conditions to be observed as most favourable to success are the following—namely, that the circulation in the limb should be for a time completely arrested, that the aneurismal sac should be full of blood, and that the circulation in the aneurism should be stopped for a sufficient time to allow the blood to coagulate.

For how long a time it may be prudent to exclude the blood from the entire limb by the Esmarch bandage, and when the more local effect of the tourniquet should be substituted for the Esmarch bandage, is a matter for further investigation. It is, however, probable, from the experience of long operations for necrosis performed under the Esmarch bandage, that we have not yet reached the limits of safety as regards the time during which the bandage may remain on the limb. One would suppose—though I daresay without sufficient grounds—that it

would always be prudent to empty the arteries of the limb by the application of the elastic bandage before adjusting the elastic ligature above.—*Lancet, May* 26, 1877, *p.* 750.

41.—VENOUS CIRCULATION IN RELATION TO SOME OF THE DISEASES WHICH AFFECT THE LOWER LIMB.

By JOHN GAY, Esq., F.R.C.S., Surgeon to the Great Northern Hospital.

In a contemporary journal, Dr. Owen Rees has contributed a "note" on gout, in the course of which he adopts the theory that the phenomena of an acute paroxysm are best explained on the hypothesis that the disease is connected essentially with the circulation in the veno-capillaries of a blood-poison. But unless some satisfactory reason be given why this poison should select those in a certain and defined region—viz., the tissues about and within the metatarso-phalangeal articulation—in which exclusively to display its specific energies the hypothesis is not only weak at one of its most salient points, but invites criticism at all. I venture to think that a reason can be supplied from the resources of an anatomico-physiological formula which may render that hypothesis unassailable. I must, however, take the liberty of referring to one point in Dr. Rees's note, as it stands somewhat in the way of the explanation I am about to offer; but must premise by calling attention to some well-known historical facts, which will also be found to bear on the subsequent considerations. Capillary phlebitis is not a newly-discovered disease. It was ably described and illustrated by Cruveilhier in his great work on Morbid Anatomy (1829-43). Nor is it new that acute attacks of gout are associated with the presence of an excess of urea, in the form of urate of soda, in the blood and tissues of the affected part. Dr. Garrod showed, moreover, most conclusively, that during such paroxysms the kidneys eliminate less than the normal quantity of urea, making it probable that its production in the blood is vicarious of the uric-acid-secreting function of the kidneys (Med.-Chirurg. Trans., 1848).

Since that time, Warren, Mackenzie, Sir Jas. Paget, and others, have particularised and described another kind of phlebitis, which is also in some way related to gout, or rather, as Sir James most aptly puts it, a phlebitis associated with ordinary gouty inflammation in the foot or joints, and which occurs with little or no provocation in persons of marked gouty constitution or with gouty inheritance (see St. Bartholomew's Hospital Reports for 1868, and Clinical Essays and Lectures, 1876). In my Lettsomian Lectures (1868) I also drew attention to the same, as well as to a rheumatic form of phlebitis—i.e.,

of phlebitis occurring not unfrequently in gouty and rheumatic subjects,—cases of which I had noted down in the course of my search after the etiology of varicose diseases, in the production of which inflammatory action in the coats of the veins, however derived, plays a predominant part.

Now, in part, it seems, in consequence of a perusal of Sir J. Paget's lecture "On Gouty Phlebitis," Dr. Rees arrived at the conclusion that the disease (an acute attack of gout) beginning in the elaboration of a poison from the urinary materials found in the blood, the venous capillaries inflame, and, as a consequence, severe pain is experienced, which is relieved immediately when effusion and swelling occur. Thus taking Cruveilhier's Phlebitis as a disease, and Garrod's hyperureic blood as an excitant and specialiser, and by bringing them into relation with each other as cause and effect, the phenomena of an attack of gout are explained; and, although the logic and the inference are indisputable, it can hardly be doubted that Dr. Garrod clearly saw both the one and the other when with marvellous skill he tracked out in the blood and tissues of a region that had been the seat of such paroxysm, not only a toxic material, but that very material whose known qualities could alone have caused it. Dr. Rees, however, does not appear to base the general construction of this theory upon the important facts just alluded to, which would have rendered it, to a certain degree, incontrovertible, but upon the very questionable assumption that gouty phlebitis is due to precisely the like conjunction of elements as acute gout, substituting in the former the tissues of the veins for those of the veno-capillaries of the latter disease. For, it is argued, if the large veins are prone to attacks of inflammation in gouty persons, why should not a fit of gout be legitimately accredited to veno-capillary inflammation occasioned by urinary blood-poison? The term "a gouty person" is certainly used in speaking of the cause of phlebitis, but, from the inference, it is in the sense that gouty phlebitis is also occasioned by the contact of poisoned blood with the affected vein-coats.

It is to that part of the hypothesis that I think an objection can be fairly taken, which regards the two diseases spoken of as identically the same. In the first place as the veno-capillaries of the affected part are the seat of the diseased action in an attack of acute capillary phlebitis, so, in order to such a conclusion, it must be shown that, in an attack of gouty phlebitis, the like vessels belonging to the vasa vasorum of the affected vein are similarly disordered; and moreover a reason given why the blood, hyperureic throughout the venous system, is toxically so in those vessels, and only over a limited area.

Although I have almost illimitable faith that ultimately the progress of discovery in the science of disease will point to a kind of humoralism in which the veno-capillary system and its contents will be found to play the principal and almost sole part in the development of morbid processes and products, and, moreover, although I have seen abundant evidence in the coats of veins of their having severally undergone many of those organic changes that are common to the like tissues in other parts, I do not know that any satisfactory evidence has been obtained in favour of the view which associates with gouty phlebitis the pathology that belongs to gouty capillary phlebitis; and, in the absence of such evidence, it is certainly difficult to say how it could happen that a certain patch of capillaries in any vein-wall should be exclusively subject to the irritation of a poison that is, or at all events is reasonably supposed to be, diffused throughout the vein-blood.

Then, again, the diseases differ in almost every point, with the exception of their being inflammatory in their nature—a matter of very little differential import. Gouty phlebitis attacks a vein, or portion of a vein, without any apparent assignable reason for the preference; whereas a veno-capillary phlebitis of a gouty character confines itself, as a rule, to the capillary system of the tissues around or within the metatarso-phalangeal articulation of the great toe, or in joints in which the vascular system, especially in relation to the venous department, is, in its arrangements, in conformity with those of the joint alluded to. The same remarks apply to other alleged gouty affections—those of the stomach, bronchi, &c. No good reason is given why these should be exceptionally excited to gouty disorder by an irritant which is simultaneously brought into contact with every tissue throughout the body.

The points are these :—During an attack of acute gout the venous blood is throughout impure. Its noxious properties attest themselves *generally* by vascular excitement, with malaise or pyrexia; *locally* by an exaltation of the same condition to a state of extreme tension in the toe; whilst the superficial veins, unless these are, as is sometimes the case, but very feebly developed, are notably bloated over the goutily affected textures and for some distance up the leg, sometimes even to the thigh. Gouty phlebitis, on the other hand, manifests itself in the vein coats, irrespective of any particular locality; is attended with an amount of constitutional disturbance much in conformity with the degree of the concurrent local irritation; and the blood is, I believe, not known to be ureic to a poisonous or hurtful extent.

These differences entitle this latter disease to a distinct place in nomenclature, but only on condition that it has an etiology

as well as a pathology, in concurrence with its symptoms, peculiarly its own, and in very limited affinity with those of acute gout.

Whilst, then, gout is due to blood poison, gouty phlebitis is or may exist entirely independent of it. It is due to a gouty diathesis or constitution, hereditary or acquired, by which a free elaboration of urea or its salts may take place in the blood, and cause an acute paroxysm; or, without any such blood impurity, may give rise to local inflammatory disorders on comparatively slight provocation, especially in the vein coats, whose morbid propensities make them a comparatively easy prey to inflammatory action from a variety of constitutional causes.

The gouty diathesis gives a pretext for phlebitis, but of itself no occasion for acute gout. It predisposes to either, but in order to the production of the latter there must be at the same time the faulty condition *plus* faulty blood.—*Lancet, May* 19, 1877, *p.* 712.

ALIMENTARY CANAL.

42.—ON CLEFT PALATE.

By Francis Mason, Esq., F.R.C.S., Surgeon to St. Thomas's Hospital.

I have no hesitation in stating that operations on very young children are, as a rule, extremely unsatisfactory, and this is the experience of other surgeons; thus, M. Passavant operated on five children varying from six weeks to two and a half years without one success. Langenbeck operated at five months and two and a half years without a better result. Billroth operated on a child two months old, who died six hours after the operation, and Rouge operated on a child six weeks old, who died twelve days after staphyloraphy.

The surgeon must, of course, be guided by the peculiarities of each case. I have myself operated on very young children in several cases, and, as just stated, one child was two months old; but this case was a very favourable one, for the fissure only involved the soft palate. I am, however, inclined to think that unless there be good reasons for doing it, the operation should not be undertaken before the age of five or six. Langenbeck advises staphyloraphy "not under seven years." Any one interested in the subject has only to look at the fissured palate in a newly born infant, and he will see how extremely thin the mucous lining is. It is almost like tissue paper, which with the slightest touch of the finger-nail will break away. I myself cannot conceive that operative measures under such circumstances can be of much avail. If the case be

watched (and I have now some twelve or more cases under my personal observation), the gradual and slow development of the soft palate, as well as the covering of the hard palate, into a tough, thick, and solid structure may be readily observed. Chloroform necessarily is a great boon during the operation, but in very young children the chief difficulties arise in the after-treatment of the case. From sheer ignorance the little patients are apt to do something that promotes disunion; perhaps they will cry perpetually, or cough, or sneeze, or play with the ligatures with their tongue, and such acts favour the separation of the parts.

I would here mention that if the wound bursts open, the surgeon should never despair of getting considerable, if not complete union, provided the smallest portion of the edges can be got to adhere. The persevering application of strong nitric acid will promote granulations, and I have seen surprising results in cases which I at first regarded as hopeless. I am especially reminded of one case, that of a boy, aged four, upon whom I operated rather more than two years ago, and who was going on quite well until one day he gave an unlucky cough. The whole of the soft palate gave way, but by using the nitric acid a most perfect cure was effected.

In describing the various operations, it will be convenient to divide them into two classes:

1st. Including those cases in which the soft palate alone is involved. This operation is termed staphyloraphy (σταφυλή, the uvula, ῥαφη, a seam).

2nd. Including those cases in which the hard palate is more or less implicated. These may be remedied by two modes of procedure:—(*a*) By stripping off the soft tissues from the hard palate (in one or more ways), and so closing the aperture. This operation is termed uraniscoplasty (οὐρανίσκος, palate, πγασσω, I form. (*b*) By completely dividing the bone and so uniting the sides of the fissure. This operation is called osteoplasty (ὀστέον, bone, πγασσω, I form).

1. *Staphyloraphy.*—Before proceeding with this subject it is necessary to make a passing allusion to the numerous mechanical appliances that have been employed in cases in which the patients have either objected to, or the condition of whose palate has rendered it not amenable to surgical treatment. The practical surgeon is aware that the best constructed apparatus cannot take the place of operative procedure. Most of the instruments that have been suggested have had for their object the closure of holes or perforations, especially of the hard palate acquired by accident or disease. Such instruments are termed obturators, and to these I shall presently refer.

Various mechanical means or false palates have been employed by Stearns, Kingsley, Sercombe, Ramsay, and Oakley Coles, as a complete substitute for an operation on the soft palate. In a patient who was exhibited at a meeting of the Royal Medical and Chirurgical Society, November 27th, 1866, "the instrument consisted of a piece of hard vulcanite with two teeth attached to the anterior portion. This supplied most accurately the deficiency in the hard palate. The fissure in the soft palate is closed by means of a piece of soft vulcanite attached to the hard, which is capable of being pressed slightly upwards and downwards by the muscles of the fauces, thus effectually closing the passage of the nares during speech or deglutition."

An ingenious instrument has been used by Mr. A. T. Norton in a case of partial cure after operation, a description of which will be found in the Medical Press and Circular of April 19th, 1876.

Mr. James Salter gives an excellent description of an instrument he has devised for the same purpose; and Mr. George Parkinson, who has had considerable experience in such cases, refers especially to this method of treatment. On the other hand, Mr. William Donald Napier, after numerous trials in such cases, has arrived at the conclusion that the value of mechanical apparatus is very much overrated, and is of opinion that no artificial means should be employed excepting in those cases in which it is not possible to perfect a cure by surgery.

Again, in order to avoid the use of cutting instruments, various means to establish inflammation and thus to produce a raw surface have been suggested. Graëfe used caustic potash, and also sulphuric acid, Ebel advised the tincture of cantharides, and Doniges used a hot iron (A.D. 1823).

Dupuytren, Béclard, and Wernecke tried cauterization by means of muriatic and sulphuric acid, but the results were not favourable,

During the past year I have been trying the application of strong nitric acid to the fissure, and, I think, with decidedly good results. The only drawback is that the process of cure is somewhat tedious. The *modus operandi*, as I explained in the Lancet, July 29th, 1876, is this: I first produce a raw surface by carefully applying with a stick (not a glass rod) the acid. nitric. of sp. gr. 1·500, and in a few days afterwards I use in the same way the acid. nitric. sp. gr. 1·420 (Ph. Brit.), about twice a week to the part, especially to the fork of the cleft. The merits of this procedure have been put to the test by other surgeons. Thus, Mr. Charles Gaine, of Bath, writes to me under date November 26th, 1876, respecting one case, that "The fissure was nearly closed after eight or ten applications of the acid. sp. gr. 1·500, and about six of the acid. nit. pur."

Mr. H. G. Armstrong, too, of the Royal Berks Hospital, Reading, states that in one case in which he applied the treatment he was quite satisfied of considerable improvement.

M. Jules Cloquet, like myself, seems to have been fairly satisfied with this mode of treatment, and in 1855 published an essay entitled "Mémoire sur une Méthode d'appliquer la Cauterisation aux divisions anormals de certain organes, et specialement a celle du vois du Palais," in which cases are given of success after repeated cauterizations.

At the meeting of the Academy of Sciences of Paris of the 21st of May, 1860, a case was brought forward by Professor Benoît, of Montpellier, which had been treated by this method. The child was eleven years old, the soft palate was completely cleft, and all the usual symptoms were present. The treatment lasted nineteen months, with two rather long interruptions. The whole cleft has now united save that of the uvula, and this result was obtained by thirty-three cauterizations, fourteen with the acid nitrate of mercury, and nineteen with the solid nitrate of mercury.

Mr. Tyrrell reported a case in which he closed a small congenital aperture of the roof of the mouth (a very rare deformity), situate about the centre, in a girl seventeen years old. The hole was only large enough to admit the blunt end of a probe, and it was cured by a few applications of a hot iron.

I may say that in selecting the kind of anæsthetic it is well to bear in mind that ether excites the salivary secretion. I therefore prefer chloroform in all operations about the mouth, and am supported in this opinion by Mr. Charles Moss, whose great experience as a chloroformist in such cases enables him to speak with authority.

The operation on the soft palate is sufficiently easy, and may be thus performed:—Although some surgeons prefer the upright posture, there is no doubt that the recumbent position is the best both for the patient and the surgeon. The patient's head can be more readily steadied, and the light directed more completely into his mouth. Under chloroform the patient is very apt to struggle occasionally, hence his movements should be restrained by straps applied in the following manner:—The knees are kept down either by a strap or bandage, which passes under the operating table. Another strap or bandage is fastened to one wrist, say the right, and is then carried under the left thigh of the patient and then secured to his left wrist. These straps, it must be understood, need not be put on too tightly; they are only intended to check movement, and should be applied in such a way as to allow of the patient being turned on his side if necessary, so as to clear the throat in case of vomiting. The mouth should be kept open by a gag of some

kind. Surgeons have their own fancies on this point. Mr. T. Smith's ingenious instrument is useful, but the instrument I am in the habit of employing and which answers the purpose remarkably well, is one that was made for me in 1870, and which has since been slightly modified by Sir William Fergusson. I was not aware, until my attention was directed to the fact by my friend Mr. Alfred Coleman, that he had devised a somewhat similar, but rather more cumbersome instrument to that first made for me.

I am quite convinced that the main difficulty in operations on the palate is the hemorrhage, which is occasionally very troublesome; and whilst I think most highly of Sir W. Fergusson's plan of dividing the muscles, yet I am inclined to believe that this part of the operation, inasmuch as it is attended with some bleeding, had better be postponed until after the denudation of the edges. Now that chloroform is so universally administered the operator is enabled to pare the fissure rapidly and generally in one continuous piece, the anæsthetic preventing the sudden contraction of the muscles. The different methods of dividing the muscle will be referred to presently.

The instruments to be employed should be of the simplest character. Roux evidently had a horror of complicated surgical apparatus. He says, after an experience extending over nearly half a century, "Je crains toujours dans la pratique de opérations les instruments qui tiennent trop du jeu des machines. Partout où les actions simples peuvent suffice, c'est de ce côté que sont mes prédilections."

The necessary preparations having been made, there should be two or three assistants to hand instruments and to soak up the blood with clean sponges, which latter should be about the size of a walnut. The plan I adopt may be thus described:—The operator, standing on the right side of the patient, commences by seizing with a pair of hook-forceps, a little below the centre of the cleft on the patient's left side. A knife is then made to transfix the margin of the cleft, and is carried downwards to the extreme point of the uvula. The instrument being now reversed, pares the remaining part of the edge upwards towards the junction of the fissure, where puckered up it remains until the other or right side is denuded in like manner. If possible the whole of the edge should be removed in one continuous piece from side to side, in order to insure the certainty that not the least particle of mucous membrane is left, otherwise perfect union cannot possibly take place. Some surgeons use scissors to denude the edges, but with such an instrument the parts are more or less bruised. A needle, armed with a thread, is then passed through the palate at about a quarter

of an inch from the free edge. The thread is then grasped with either the hook forceps or with a pair having serrated blades and the needle withdrawn. The needle is now re-threaded (or another may be used), and is to be passed through the opposite side exactly on the same level. If now the end be passed through a loop, and traction made, the end will be brought through the opposite side of the fissure. It now only remains to pull through one side of the thread.

When sufficient threads, say three or four, have been introduced, the next step is to approximate the edges. A slip knot is perhaps the best; and before putting the end into the noose, it is well for the surgeon to take the other end of the thread in a figure-of-8 form around his left forefinger and thumb, which manœuvre prevents the thread from getting entangled, and then it runs as easily as possible. Coloured threads may or may not be used. It is, I think, a good plan as the operator proceeds to tie the ends of each succeeding thread; and supposing four sutures are employed, the practice I adopt is to give the first thread, or that nearest the hard palate, to an assistant, who holds it at the centre of the forehead; the second is held *over* the patient's ears, the third *under* the patient's ears, and the fourth at the sides of the neck. Simple as this proceeding may appear, it saves confusion to a marked extent, for when the time arrives for drawing together the sutures, there is no difficulty whatever in selecting the corresponding ends. As a rule, I secure the stitches from above downwards. The operation is completed by either dividing the muscles, according to Sir W. Fergusson's plan, before the sutures are closed, if this has not already been done, or by taking the tension off the stitches by making a vertical incision, as Dieffenbach did, about a quarter of an inch in length on each side of the fissure. When necessary the anterior and posterior pillars of the fauces, with some fibres of the palato-glossus and palato-pharyngeus, may be divided.

Respecting the operation a few practical points may not be out of place. There is often some difficulty in grasping the thread when passed through by the needle, but it may be easily secured if the needle be thrust freely and somewhat roughly through and slightly withdrawn at once; but this must be done immediately and before the thread gets saturated with moisture. A slight loop in the ligature is thus formed, which can be readily secured in the grasp of the forceps. Various instruments have been devised to catch the thread, but they are unnecessary, as a pair of forceps such as those already referred to answer the purpose prrfectly. It is important, too, not to draw the stitches together too tightly, for there is generally a little swelling after the operation, and allowance must be made for this, but

in referring to this point it must be distinctly understood that the edges must be applied with the most perfect precision, otherwise union cannot be expected. It is desirable, also, to place the knots so that they shall not lie exactly over the wound. In order to increase the breadth of the raw surface I have seen Sir William Fergusson take a curved knife and run it along the cut surface. This he thinks gives a greater probability of union.

As to the length of time the stitches should remain is a point on which there is much difference of opinion. Sir W. Fergusson advises their removal about the third or fourth day; but then he was guided by circumstances, and has left them even to the eleventh day. I myself leave them to work their way out, unless they appear to cause irritation, when they ought to be taken away immediately. A remarkable case bearing on this question was under my care in February, 1876:—The patient was a boy aged 14; the stitches were left for one week, when a blush appeared all over the palate, and I was fearful that the parts might burst open. I removed the sutures, and on the following day all the inflamed appearance was gone, and the fissure united most perfectly.—*St. Thomas's Hospital Reports, vol.* vii., *p.* 61.

43.—ON THE TREATMENT OF NASAL POLYPUS.

By Dr. Morrell Mackenzie, late Physician to the London Hospital.

As soon as the existence of a polypus has been ascertained, it is advisable at once to effect its removal by some means, as delay only renders extirpation more severe and difficult. In the case of mucous polypi we may find it advisable or necessary to follow any one or more of five different courses of treatment viz., the application of drugs, manipulation, galvanic cautery, evulsion, or enlargement of the outlets of the nasal cavity.

Application of drugs.—This, as may be supposed, was one of the primitive methods of getting rid of polypi. It is, however, of no value except when the growths are very small and when they can be easily reached by the remedies employed. Small polypi may be painted frequently with strong astringent solutions, (Pigmenta, T. H. Ph.), and chloride of zinc or perchloride of iron is well adapted for this purpose. The author has found the best results from the use of the latter salt mixed with just sufficient water to form it into a thick paste. Bichromate of potash and nitrate of silver have also been tried frequently with more or less success. Mr. Bryant thinks highly of the insufflation of finely-powdered tannin into the affected nostril. Rauchfuss' insufflator is a good instrument for making this

application. Another remedy of a different kind is that introduced by Primus of Babenhausen—namely, the saffronised tincture of opium of the Prussian Pharmacopœia. If the growth be painted several times a day with this liquid, in about a week or ten days the polypus, under favourable circumstances, becomes shrivelled up and falls from its attachments.

Evulsion.—Of all the methods this is by far the readiest and the most generally adopted, although the injudicious application of it has occasionally led to its being decried. It has the disadvantage of being often extremely painful, and is commonly supposed to give rise to copious and even dangerous hemorrhage. My own experience, however, concurs with that of Pott, as I have never met with cases where the treatment gave rise to serious bleeding; on the contrary, if the hemorrhage did not soon cease spontaneously, it was immediately subdued by the use of the ordinary hæmostatics.

(*a*) In my opinion, no method of removing mucous polypi is equal—in simplicity, certainty, and rapidity—to evulsion by means of forceps. This practice, which seems to have been introduced by Arantius towards the end of the sixteenth century, has, since that time, found more favour with surgeons than any other measures, for operating on these tumours. Unfortunately its great success and facility of execution have often led practitioners to suppose that the proceeding was equally applicable to all intra-nasal growths, wherever situated, and whatever the nature of their attachments. Acting on such premises we find surgeons increasing the size and leverage of their forceps, and adapting them by suitable curves for introduction either by the nostril or through the mouth, as if no more consideration were necessary in the efficient practice of the operation than to seize every nasal tumour with tenacity, and wrench it away with violence. The consequences were often deplorable, and we learn from Pott that the tearing away of the septum, or the greater part of the palate bones, was a not infrequent accompaniment of the extraction of polypi by such inconsiderate surgery.

The instrument which I have found most generally useful is a modification of my crushing laryngeal forceps more slenderly made, and with shanks curved so that the handles do not obstruct the operator's view. A pair of slender forceps, however, with well serrated blades, slightly curved, and not much larger than an ordinary pair of scissors, often answer well. If the growth is of moderate size, and a good view of it can be obtained from the front, the nostril may be kept open with Fraenkel's speculum, and the pedicle easily seized. The base of the growth should first be well twisted backwards and forwards, and then torn out. It is advisable always to make the twisting of the peduncle the first step of the extraction, as by this means the

deep attachments of the tumour are more likely to be torn completely out. When the polypus is situated far back in the nasal channel, the speculum will be of no service, but we can command the growth by passing the forefinger of the left hand round the soft palate into the posterior nares. The forceps introduced from the front, as before, can then be guided so as to obtain a good grasp of the peduncle, and the rest of the operation remains as above described. When the polypus is very large, and attached at several different points, it may be extracted in successive portions, or we may resort to the expedient devised by Richter, of using an instrument made somewhat after the pattern of midwifery forceps. In this way we can adjust one blade into position, and then introduce the second, when both can be locked together and a secure hold taken of the tumour. In rare cases we may even have recourse to some of the leviathan forceps forged by Thomas Whately, to which some reference has been made above. The structure of a mucous polypus is usually so soft and tender that it breaks down and tears under the influence of a very moderate degree of traction. On this account, the method followed by Dzondi, of drawing the growth forwards, so as to put the peduncle on the stretch, with one pair of light forceps, and then with another pair squeezing and bruising the root of the tumour as close as possible to its attachments until it gives way, is sometimes practicable, although the advantages of such a proceeding are not obvious. The inventor, however, claims for his device that it ensures a perfect freedom from all danger of recurrence of the growth, as the deep attachments of the growth are by this means entirely uprooted.

(*b*) The evulsion of polypi, by snaring them in a noose of string, and then employing traction, was no doubt constantly practised at a date anterior to Hippocrates. The instrument invented by Mr. Hilton is the modern modification of this rude method, and may often be employed with advantage. In using this snare the polypus is embraced as near as possible to the base of the pedicle by a loop of wire, which is then drawn home and tightened by means of a cross-piece sliding on the stem of the instrument. Sufficient force is then applied to tear the growth from its attachments. The difficulty consists in getting the loop of wire over the body of the tumour and well round the peduncle. This object may occasionally be effected by the aid of a small blunt fork, which, being passed into the nostril, can be made to direct the loop over the thick extremity of the growth. Mr. Durham has used Mr. Hilton's snare extensively at Guy's Hospital with remarkable success. It does not occasion much pain or hemorrhage; the growth in most instances is very completely extracted; and, of course, after a little practice, the

operator becomes skilful and expeditious in applying the wire. When the polypus is situated near the pharyngeal end of the nasal channel, we may have recourse to the use of Bellocq's cannula, as first suggested in such cases by Waldenburg. A stout silk thread should be fastened to the extremity of the cannula, and passed into the pharynx in the usual way. One end of the thread being retained in the mouth, the sound is withdrawn. The wire of the snare is then attached to the string hanging from the nose, and, by means of the string projecting from the mouth, is drawn to the back of the nares behind the polypus and pushed well over its body with the assistance of one of the fingers introduced through the mouth. —*Lancet, July* 28, 1877, *p.* 118.

44.—TREATMENT OF OZÆNA BY A NEW OPERATION.

(Case under the care of Mr. HARRISON CRIPPS, at the Great Northern Hospital.)

A. B., aged thirty, probably syphilitic, struck the bridge of her nose five years ago in falling against the corner of a table. She suffered considerable pain at the time, and hemorrhage was profuse. The nose remained tender and swollen for two or three months, and the nasal passages became so much obstructed that she was scarcely able to breathe through them.

Six months after the injury a fetid discharge from the nostrils commenced, and continued up to November last, when she first presented herself at the hospital. The nose was at that time considerably flattened, and both nostrils were reduced to little more than pin-hole apertures. The soft parts between the nose and upper lip were deeply ulcerated, and flowing over this ulcerated surface from the nostrils was a thin ichorous discharge. The fetor arising from the discharge was beyond all description, and pervaded the room to an extent scarcely bearable. She complained bitterly of her condition, being an object of aversion to all her friends. The introduction of a probe into the nostrils was a matter of some difficulty, and caused much pain. On the first examination no dead bone could be detected. The patient was ordered full doses of the iodide of potassium, and directed to wash out the nasal cavities thoroughly with Condy's fluid, applied after Thudichum's method. She continued under observation till the middle of January, but without any marked amelioration in her condition. As dead bone could at this time be detected with a bent probe, and the patient was anxious that something should be done to relieve her, she was operated on according to the plan advocated by Rouge, of Lausanne.

Chloroform having been administered and the patient placed

on her right side, the right corner of the upper lip was seized by the operator, and the left by his assistant; by this means the lip was everted and drawn upwards, while the soft parts were separated by a clean sweep of the scalpel, cutting upwards with its edge kept close to the bone. This incision extended from the second bicuspid tooth on the right side to that on the left. By drawing upon the upper lip, the nose, together with the soft parts forming the anterior portion of the face, could be easily raised in such a manner as thoroughly to expose the nasal fossæ. A large quantity of dead bone could now be both seen and felt. The dead portions of the bone were quite loose, and were easily removed with the finger and polypus forceps. After the removal of the bone, the forefinger could be passed quite back to the posterior wall of the pharynx, the cavity feeling soft, velvety, and entirely free from dead bone. The operation was completed by replacing the lip in its natural position and retaining it there by a single strip of plaster placed transversely across the face. There was scarcely any bleeding during the operation. The wound healed rapidly by first intention, without the slightest scar or deformity. Six days after the operation a careful examination failed to detect the line of incision, so complete had the union become. The patient has been seen many times since; all discharge has ceased, the ulceration of the upper lip has healed, and there is not the least fetor to the breath. The pieces of bone removed comprised a portion of the left palate, the left inferior turbinated, and a considerable portion of the vomer. These pieces were thickly coated with a hard, mortar-like substance, exhaling a most fetid odour.

That this method of operating is one of the greatest value and importance cannot be doubted, and would seem justifiable not only in cases of ozæna, but also in some cases of polypi or other growths, for the entire removal of which a considerable space and more complete view are necessary.

Ozæna, according to the most recent observations, is dependent in the large majority of cases on a sequestrum or carious portion of bone, and it is not impossible that some of the remaining cases, supposed to be due to a constitutional cause, really arise from a local, although undetected, source of irritation. For many of the cases narrated by Rouge an examination prior to the operation had failed to detect a cause, but after the anterior nares had been exposed in no case did he fail to find a sequestrum or carious portion of bone. In one instance a portion of the bony septum was found dead, lying between the two layers of mucous membrane, a condition of things which accounts for its not being detected prior to the operation.

In the case above narrated it was not found necessary to cut through the cartilaginous septum at its attachment to the anterior nasal spine, it having been previously destroyed by disease, but in ordinary cases before the nose can be lifted this should be done with scissors.

The ease with which the operation was performed, the thorough exposure of the nasal fossæ, the absence of hemorrhage, and the beneficial results obtained entirely agree with the cases described by Rouge.—*Lancet, May 5*, 1877, *p*. 643.

45.—A NEW METHOD OF CURING RUPTURE.

By Dr. G. Heaton.

In the Boston Medical and Surgical Journal, the author describes the result of a method of treatment based upon experience derived from many hundreds of cases of hernia, including inguinal, femoral, and umbilical enteroceles and epiploceles, reducible and irreducible, and extending over a period of more than thirty years. His method may be briefly described as consisting of a mild irritation of those portions of fibrous tissue lying directly in contact with the exterior of the neck of the hernial sac, thickening and consolidating their substance and effecting a contraction of the openings. The irritant used in this operation is made by dissolving with the aid of gentle heat fourteen grains of the alcoholic solid extract of white oak bark in half an ounce of Thayer's fluid extract of the same drug. With an instrument somewhat resembling a hypodermic syringe, about ten minims of this preparation are slowly deposited in such a manner as to moisten all the fibrous tissues in contact with the neck of the sac, including the rings themselves. Very little pain attends or follows the operation. A light truss or firm bandage is immediately applied and ordered to be worn a longer or a shorter time, according to the severity of the case. During the first week after the operation, the patient is usually required to remain in bed. After that time he gradually gets about, and in many cases returns to his business at the end of a fortnight. In the slight cases all mechanical support is removed in six or eight weeks, and the patient is pronounced "cured." Owing to the anatomical peculiarities of femoral ruptures they are not as readily cured as the other varieties, but Dr. Heaton claims a sufficient percentage of successful results amply to compensate both patient and surgeon for the operation. Contrary to expectation the above procedure is more successful in umbilical herniæ than in any of the other kinds. The reasons given are the greater aggregation of fibrous fasciculi which encircle the umbilicus, and the com-

plete closure of the opening. Previous to the injection for the cure of an umbilical hernia, the neck of the sac is to be separated from the edges of the aponeurotic opening by pretty forcible taxis, or, this failing, by a subcutaneous dissection. In actual practice about one-third of the cases require a repetition of the operation, and the worst ones may require several before a satisfactory result is obtained. The above method of treatment is not applicable to irreducible ruptures until the sac has been emptied of its contents. If this object cannot be accomplished by strong and repeated manipulations, Dr. Heaton opens the sac, separates the adhesions, and replaces the bowel. Should the contents be omental, and of large size, he applies a ligature and cuts off the protruding mass. When the patient has recovered from the effects of these procedures, which he is said invariably to do in periods varying from a few days to several months, the operation by injection is resorted to, if necessary. Twenty-nine successful cases are reported to illustrate this mode of treating irreducible hernia. No fatal cases are given for the reason, as he declares, that he has never met with any. There is little to be said about the pathology of this method of treatment, as Dr. Heaton has had only one opportunity of making an autopsy on a person who had undergone the operation. His theory of the action of the irritant is as follows: "The operation is subcutaneous, the parts affected are fibrous tissue, and the irritation itself is mild. The parts chiefly and immediately affected by the foreign material are, with a single exception, fibrous or fibroid tissues, receiving but little or no red blood from the circulation, and supported chiefly by the nutritive juices. The disturbance excited does not exceed a low grade of irritation, and the moderate amount of lymph thus subcutaneously produced has a strong plastic or organising tendency. Also its production is largely interstitial, thickening the fasciæ and tendons, contracting and consolidating, though not absolutely closing up the entire fabric of the hernial or inguinal canal. Owing to the mildness of the preparation of quercus alba the irritation ceases in its earliest stage, and does not increase to a real inflammation, which, if it occurred, might readily run on to suppuration. Owing, moreover, to the peculiar slowness with which all fibrous tissues when irritated recover, as well as to the powerful plastic tendency of the lymph thus generated, the thickening contraction and consolidation which follow the irritation, persist for a most remarkable length of time so long that for all practical purposes they are permanent, or at any rate sufficiently so to enable nature to re-establish herself and effect a cure of the hernia.—*Practitioner, Oct.* 1877, *p.* 296.

46.—ON THE REDUCTION OF STRANGULATED HERNIA.

By Timothy Holmes, Esq., F.R.C.S., Surgeon to, and Lecturer on Surgery at St. George's Hospital.

With respect to the methods which surgeons use, the taxis is of course the thing first employed. I will not go into the question of position—of turning patients on their head, &c. All this seems to me to depend upon an entire, I will not say ignorance, because that would be rude, but an entire forgetfulness of the obstacles which there are to reduction. Gravity is no serious obstacle to reduction. I do not think putting a man on his head makes his hernia a bit more reducible than putting him on his heels. It does not appear to me that you get any advantage by raising the feet, still less by raising the whole body up altogether so that the man stands on his head; because though you certainly may get some slight advantage from the traction of the bowels which are inside the belly putting down the bowel outside it, yet you lose a great deal more from the spasmodic condition into which you throw the muscles of the lower extremity and the abdominal wall. It seems to me an entire mistake to waste time upon any of these things. The taxis is the plan which you have first to employ, and that of course aims at neutralising the first of the obstacles that I mentioned. When the bowel or the omentum has been suddenly protruded through the ring as I have said, the first effect of strangulation upon it is to cause a great increase of bulk by the secretion of air and fluid into the bowel, by the secretion of exuded matters into the tissue of the omentum, and by the detention of blood in both of them. Your object is by gentle pressure to empty the bowel of air and fluid, to empty the vessels of blood, and to push back some of the contents of the hernial tumour through the ring; and the first thing that enables you to be certain that you will succeed in that, is feeling a little air or fluid gurgling back into the cavity of the abdomen. You saw a case a few days ago of a young man who had had inguinal hernia some time, and had not taken much notice of it, till on some violent exertion the inguinal hernia protruded, and at once became acutely strangulated. He was brought in here in great pain. A good deal of vomiting had taken place before admission, and in fact things looked rather bad. There was a very tense tumour, without any impulse on coughing, but we succeeded quite easily in reducing it; by putting the man under chloroform and making gentle pressure, in the course of two or three minutes it went up. And I knew it would go up as soon as I began to handle it, by feeling a little air gurgling out of the hernial tumour into the abdominal cavity. In a case of that kind it would not have been justi-

fiable, if I had failed to reduce it, instantly to have gone on to operation. It seemed to me the strangulation, although acute, was so recent and the symptoms altogether so incipient, that I determined, if I failed in reducing it, to have put him into the warm bath, and to have waited, at any rate, a good many hours, and to have made renewed attempts at taxis, before proceeding to operation. Taxis is very much assisted by chloroform, and I think much more assisted by chloroform than by ether. I do not myself recognise that very great difference in the danger of anæsthesia from chloroform and from ether which some surgeons profess to feel. It seems to me that complete surgical anæsthesia is always a dangerous thing, whether produced by chloroform or ether; and I think chlorofom produces so much more complete relaxation, and that so much more speedily, that it is very much more convenient in the reduction of dislocations and strangulated hernia.

But although our horror of repeated manipulation in attempting taxis may have been a little exaggerated by cases which we have seen of severe injury to the intestine by the taxis—cases which I would venture to hope are somewhat exceptional—at the same time I do not think that too prolonged or too frequently repeated efforts at taxis are desirable. I think that Professor Gross, of Philadelphia, has erred somewhat on the other side in stating that almost all herniæ may be reduced by repeated manipulations. It seems to me that when vomiting is distinctly declared, and has turned from bilious vomiting into vomit mixed with the contents of the small intestine—what is sometimes called fæcal, although it is not fæcal, but where the vomit has turned from the green colour of bilious vomiting to the dirty brown of the contents of the small intestine—then you had better not wait any longer; if you cannot by a renewed attempt at taxis reduce the hernia, you must operate. But that is an interval which allows you a considerable amount of time. You do not, usually speaking, get that condition of things until after twenty-four hours of strangulation, and if you have the patient under your care during the whole of that time there is no reason why you should not put him once, if not twice, under the influence of chloroform, and attempt taxis. Besides, you should put him into a warm bath for a considerable time, or apply fermentations for several hours before you begin to think of the *ultima ratio* of an operation. Ice is very frequently used, but seems to me to be very seldom of any use, except in those cases of inguinal hernia in which the tumour is voluminous and evidently contains hardly anything except intestine, and that very much distended with air. In these cases you will find the application of ice of considerable utility.

There is another method that has been proposed for the reduction of hernia, and that is by the aspirator. Dr. Dieulafoy spoke of the aspirator at first as being almost free from danger in these cases; but that certainly seems not to be the case. The object of the aspirator is to empty the bowel, or sometimes you do not perhaps puncture the bowel, but merely empty the sac; more generally you puncture the intestine, and draw off a good deal of gas and a certain quantity of fæcal fluid. The puncture in the intestine will be but small. It will be immediately closed by the slipping upon each other of the three coats of the intestine, so that when the aspirator is withdrawn the intestine will no longer have a hole in it. The three holes in the three coats will not correspond, and you can very frequently reduce the hernia afterwards. It has appeared to us at this hospital to be a method which is dangerous, and so doubtful that we have not as yet ventured upon using it; but at the same time it has been used elsewhere in many cases, and its use would be most indicated in the case of a voluminous hernia in which ice is used. So much for the ordinary methods of reduction which precede the application of the knife.

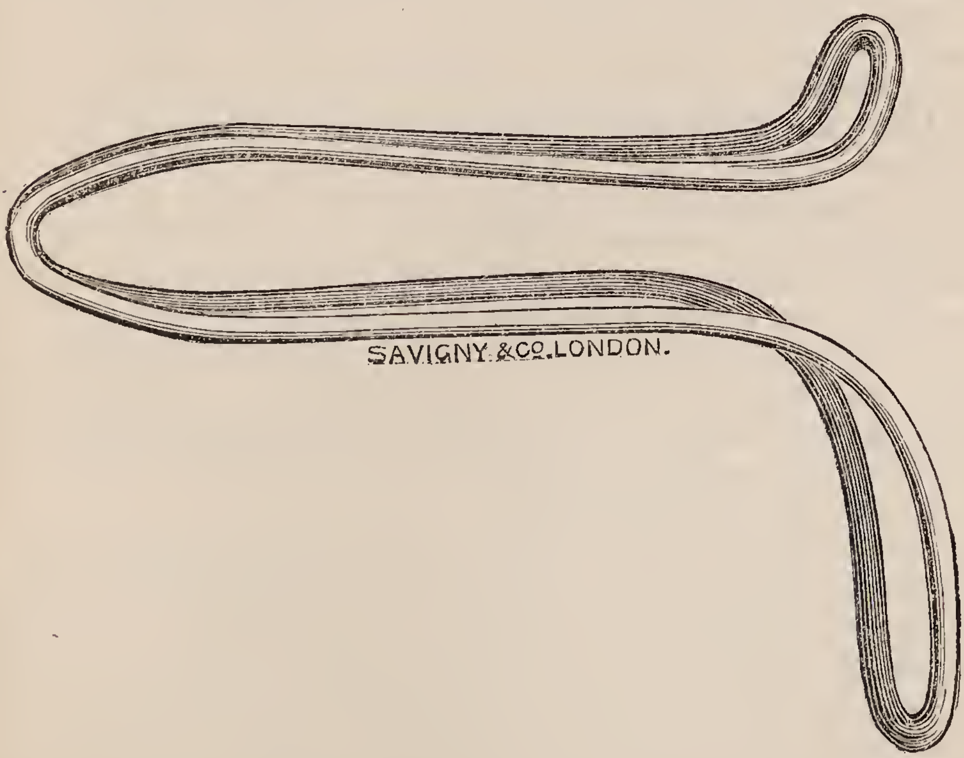
The warm bath has been lately disused at this institution; at least I think I am right in saying so. I certainly have not used it for some years. Sir James Paget, however, recommends it to be used "in all cases that are not very bad;" that is to say (as I think he explains in another passage) in all cases in which you do not think the operation is absolutely indispensable at that moment, and in many of these cases you would not use taxis at all. In many cases in which you think the case is so bad that an operation cannot be put off even a few minutes, you would also think the matter had gone on so far that you had better not handle the gut in any way. The best way is to cut down into the sac at once and investigate the condition of things. It certainly appears to me that Sir James Paget is right in that respect, that in most of these instances you wait a few hours, and if you wait a few hours I cannot help thinking the patients sometimes are a good deal relieved; the muscular spasm is considerably relaxed, and the hernia made very much more reducible by the old method of applying a warm bath, and I must say I rather regret it has so much gone out of fashion at this hospital. I will try at any rate in my own practice to bring it in again. Our plan when I was a student used always to be to put the patient into a hot bath, make him entirely faint, and then try to reduce the hernia. Sir James Paget directs you to use the bath only moderately warm instead of hot. I confess the old plan still seems to me the best. One knows the extreme relief that the hot bath pushed to faintness gives in cases of descent of a stone from the kidney, and also

in the case of a gall-stone; and we have often seen the same relief in cases of strangulated hernia.—*Medical Examiner, July 26, 1877, p.* 586.

47.—ON RECTAL EXAMINATIONS.

By Richard Davy, Esq., F.R.C.S., Surgeon to the Westminster Hospital.

Within the past month we have had a series of cases illustrating features of surgical interest in connection with the rectum. I shall first direct your attention to some anatomical facts, and then offer clinical remarks on the treatment of fistula *in recto*, fissured anus, pendulous skin-tags around the anus and piles. Hilton, in his classic work, 'On Rest and Pain,' draws attention to a white line of demarcation between the external and internal sphincters; the former I have been in the habit of describing as the sphincter ani, the latter, as the sphincter recti. They both act as janitors, but the anal sphincter specially guards against intrusion from without, whilst the rectal sphincter, on receiving the order from his superior officer, carries out the final expulsion of fæces. I apprehend that nature has erected Houston's folds on very much the same principles that barriers are erected in public places, viz., to prevent over-crowding, and



to give supportive protection to and from "the residual element." The common iliac arteries may be felt pulsating right and left of the rectal column; I have lately utilised this

anatomical fact for the restraint of hemorrhage during amputation at the hip joint; the position of the internal iliac arteries also with regard to the pelvic wall suggests that the mechanical pressure of the fœtal head and body during parturition may assist in checking flooding in cases of placenta prævia. The healthy anus is an even, tight, depressed ring of tissue, dilating easily to a diameter of one inch and a quarter, sensitively resenting intrusion from without both of solids and liquid, whereas the rectum may be distended to a diameter of four inches, and is tolerant of manipulation. Here you may examine the simple yet very effective speculum for the rectum that I am in the habit of using; it is preferable to Hilton's window speculum, because it is larger, cheaper, and exhibits the whole of the rectal walls at a glance; the instrument itself occupying the minimum of space.

Let me now draw attention to the common complaint called fistula *in recto.* In your text-books, and even in Bryant's Surgery, this complaint is erroneously called fistula *in ano;* the anus is but a linear entity, length without breadth, the mouth of the sewer. If two apertures exist, one is in the skin around the anus, the other in the lower part of the rectum. In all operations on the rectum, attend to the following hints: 1. That the bladder be empty; 2. That the banks of the sewage-canal be well washed previously with tepid soap and water; 3. That your own hand be oiled, the nails cut short, and the semilunar folds around the nails filled with soapy smears; 4. That suitable lavatory accommodation (with disinfectants) be ready for your own ablution. Divide the fistulous track accurately; retraction of the sphincters results; the floor of the sinus is free from friction, and granulations adhere from the floor towards the circumference of the anus. Administer opium *per rectum* or by the mouth as occasion seems fit, so as to quiet the intestinal tract. Nitrous oxide gas may well be used for these short operations, because sickness is a very aggravating occurrence as a sequence. In fissured anus, where exquisite sensibility occurs, and the patient carries the aspect that Professor Miller indicated as *mens conscia recti*, divide the nervous filaments in the sulcus, as explained by Hilton.

In considering the treatment of hæmorrhoids, bear in mind this standard rule, that whatever you remove as *bonâ fide* integument may safely be done by a curved pair of scissors; whatever is removed as *bonâ fide* mucous membrane must be done by clamp and cautery, or by ligature. In the out-patient department, I constantly cut off cutaneous tags around the anus with scissors, and treated the owners of them as out-patients; but never did one venture so to handle hæmorrhoids. Patients with piles require careful rest and attention prior to operating.

Mr. Henry Smith has introduced a pair of forceps for holding these slippery customers, and then burns them off; my own experience inclines me to hold that the whole curative credit is due to the cautery. At this hospital we use a much stronger clamp than Smith's, and more after the St. George's Hospital pattern, because the grip is much firmer and cannot slip. The cautery I now use is this very ingenious and practical platinum cautery invented by Dr. Paquelin; after heating the platinum-tube over a spirit-lamp, the vapour of benzoline driven over this heated surface maintains its temperature as a cauterising agent. The late Mr. Bruce devised an arrangement for utilising common gas for cautery purposes, but benzoline is more portable than gas. No modern invention has succeeded so well in neutralising the apparent brutality of the hot iron as this of M. le Docteur Paquelin. These rectal wounds must all be treated by consummate cleanliness, and require no other dressing. You will find the steam-spray very grateful, and a soft hog's bristle brush an useful adjunct.—*British Medical Journal, July* 21, 1877, *p*. 72.

48.—ON THE OPERATIVE TREATMENT OF INTERNAL PILES.

By THOMAS ANNANDALE, Esq., F.R.S.E., Surgeon to the Edinburgh Royal Infirmary, and Lecturer on Clinical Surgery.

The true principle of operative interference in cases of internal piles I take to be—to confine the operation to cases in which the disease is uncomplicated with other serious affections, has resisted ordinary treatment, and is causing disturbance to the general health or comfort, either by bleeding or by constant protrusion and irritation, or both.

The principle of the operation itself is to destroy or remove simply, effectually, and without hemorrhage, the vascular growths or masses forming the piles; and in so doing to leave a sore or sores which will heal and contract safely, quickly, and thoroughly.

Although internal piles differ somewhat in their composition and appearance, it is not necessary to distinguish between the varieties as regards the question of operative interference, for practically they all require to be treated on the same principle.

The application of nitric acid may cure internal piles when there is no very distinct tumour or mass, but simply a vascular surface. This treatment is, however, unsatisfactory and ineffectual in the majority of cases, and therefore need not here be further considered. In the Lancet for 17th of February of this year, Mr. Reeves describes a method of treating the disease by inserting a heated probe, so as to destroy the substance of the pile. In connexion with this plan, I may mention that my

friend Dr. Little, of Singapore, tells me that for many years he has treated internal piles on a similar principle, by making one or more small punctures in them, and then introducing a fine stick of caustic, so as to destroy their vascular structure. That either of these methods may be successful in curing circumscribed piles is surely proved by the experience of these gentlemen; but I doubt whether they are applicable, except in a certain class of cases; and when the tumours are large, the presence of one or several piles treated in this way must lead to more or less prolonged irritation, until the cure takes place.

The two operations at present in general use for the cure of internal piles are—1. The operation by ligature; 2. The operation with the clamp and cautery (Smith's). It is between these two methods I wish to judge. There are two ways of applying the ligature in treating this disease. The first is that in which the base of the growth is transfixed by a needle armed with a double thread, and each half then ligatured separately, so as thoroughly to strangulate the whole mass. The second is that suggested by the late Mr. Salmon, and consists in separating with a pair of scissors the pile from its connexion with the muscular and sub-mucous tissues at the point where the mucous membrane joins the skin, and then applying a ligature round the upper and remaining attachment, which includes the vessels supplying the tumour. The strangulated pile may now be returned, or, if large, a portion of it may be cut off, and the stump and ligature returned.

This second method has the advantage that it allows the strangulated mass to separate more easily, and, as there is less tissue included in the ligature, the resulting irritation is less. Having practised this operation, I can testify to its advantage over the other plan of using the ligature.

When internal piles are treated by ligature in either way, the usual progress of the case is, that the ligature gradually ulcerates its way through the base of the tumour, and is detached along with the strangulated and dead portion of tissue at a period of from seven to ten days after the operation. If the tumour is small, the ligature may separate a day or two earlier, and, if large, the ligature may not come away before the twelfth or fourteenth day. During all this time there is more or less inflammatory action, with irritation of the rectum in the region of the ligature, and the presence of one or more putrid masses in the same canal. After the ligatures have separated, one or more sores result, according to the number of tumours treated, and these sores are not usually completely healed for several weeks longer.

Without the slightest doubt, internal piles of all kinds, and in patients of all ages and conditions, can be successfully

removed by means of the ligature, and I could record numerous interesting cases treated in this way with a good result; but the operation by ligature has some disadvantages and risks, to which I will now refer. These disadvantages are :—

1. That the application of the ligatures gives rise to irritation more or less severe, and lasting usually for two, three, or more weeks.

2. That confinement to bed is, in the majority of cases, necessary, until the ligatures separate.

3. That the resulting sore or sores do not usually heal quickly, and are attended by more or less suppuration until cicatrisation takes place.

4. That the operation is sometimes followed by inflammation and suppuration in connexion with the lymphatics, and also by the formation of ulcers or sinuses, which require further operative treatment.

The principal risk is pyæmia. This risk cannot be said to be great, or at all common; but in about two hundred cases of this operation, I have met with at least four instances of fatal pyæmia.

When piles are removed with the clamp and cautery, the general progress of the patient is as follows :—On the third or fourth day after the operation, the patient is able to leave his or her bed, and at the end of a week or ten days to be out of doors. For the first four or five hours after the operation, there is usually pain and irritation, which varies in intensity in different patients, and this pain generally recurs at the time of the first opening of the bowels, but there may be little or no pain from the first. From my observations, I should say that, as a rule, there is certainly much less pain and irritation after this operation than after the application of the ligature; and, from the nature and speedy healing of the resulting sore in the former operation, these symptoms are rarely present after the first week or ten days, if the clamp and cautery have been used.

In illustration, I give brief notes of the following four cases, in which the piles were removed by means of the clamp and cautery.

Case 1. Mr. C., æt. 55, was operated upon in June 1872, and two internal piles, the size of filbert nuts, removed with the clamp and cautery. The patient's bowels were moved on the third day after the operation, and on the following day he was out of bed. On the ninth day after the operation he was out of doors and quite well.

Case 2. Mr. M., æt. 60, was operated upon in May 1873, and four large internal piles removed with the clamp and cautery. The patient made an excellent recovery, and was out of doors well at the end of two weeks after the operation.

Case 3. Mr. R., æt. 30, was operated upon in June 1875, and three internal piles, the size of filbert nuts, removed with the clamp and cautery. The patient was out of bed on the fifth day, and out of doors on the tenth day after the operation.

Case 4. Mr. U. was operated upon in September 1876, and three internal piles, the size of small walnuts, removed with the clamp and cautery. He was out of bed on the fourth day, and out of doors at the end of one week after the operation.

Since 1872 I have operated with the clamp and cautery on twenty-four patients, and all, with one exception to be again referred to, have progressed as in the cases just related.

The advantages of the clamp and cautery, as compared with the use of the ligature, are, in my opinion, as follows:—

1. By means of the clamp and cautery the piles are at once removed, and do not remain in the rectum as dead and putrid masses.

2. The irritation and pain are not so severe or so prolonged as in the operation by ligature.

3. The patient's confinement to bed and to the house is much shorter.

4. The resulting sores heal more quickly, and are attended with less risk of suppuration and its attendant local and general dangers.

It so happens that I can offer some strong evidence in favour of the clamp and cautery in connexion with the amount of pain and irritation following the operation, and the quickness of recovery after it—for, in three of my cases operated upon in this way, the patients had previously undergone the operation by ligature. The testimony of all these patients who had experienced both methods was most strongly in favour of the clamp and cautery.

I give a short note of these three cases.

Case 1. In 1869 I assisted the late Mr. Syme to operate on Mr. W., æt. 40, for internal piles. Three tumours were treated by ligature, and he was confined to bed for nearly three weeks after the operation. It was fully six weeks before he was able to go about comfortably. In April 1873, he consulted me on account of a return of the disease, and I removed with the clamp and cautery two tumours, the size of filbert-nuts. The patient was out of bed on the fourth day, and out of doors on the sixth day after the operation. He described the second operation as "paradise" compared to the first one by ligature.

Case 2. Dr. —— was operated on by the late Mr. Syme with my assistance in 1868. Last year, he consulted me on account of a return of the disease, and I removed with the

clamp and cautery two internal piles the size of small walnuts. Shortly after the operation this patient got out of bed, contrary to orders, and strained at stool. The result was some bleeding, which, however, was checked by the introduction of a plug of lint into the rectum, and gave no further trouble. He was out of bed on the fifth day, and out of doors on the tenth day after the operation. Mr. Syme's operation on this patient was by ligature, and it was then two weeks before he left his bed, and a month before he was out of doors. He expressed his strong opinion in favour of the clamp and cautery.

Case 3. In 1867 I assisted the late Mr. Syme to operate upon Mr. ——, æt. 53. Two large internal piles were removed by means of the ligature, and the patient was confined to bed rather more than two weeks after the operation, and was not out of doors for three weeks. Six months ago he consulted me for a return of the disease, and I removed with the clamp and cautery two internal piles, the size of filbert-nuts. He was out of bed on the sixth day after the operation, and he has recently written me as follows in regard to the two operations:—"As to the two operations, there is no comparison, the last (that with the clamp and cautery) was attended with the least sufferings, and the after-effects much better and easier than the former. It is a far superior method."

I have had similar evidence from a fourth patient, whom I assisted the late Mr. Syme to operate on by ligature, and who had a second operation performed by Mr. Smith himself, with the clamp and cautery.

I would here ask, Are there any risks connected with the use of the clamp and cautery? One of the principal objections which has been brought against this method is the risk of hemorrhage after the operation. If the cautery or heated knife be properly used at an almost black heat, and ordinary precautions taken after the operation, I consider that the risk is a very slight one indeed. There has been hemorrhage in only one of my cases—to which I have already referred—and there was good cause for its occurring. Is this operation entirely free from the risk of pyæmia? Cases have occurred, and have been reported, in which fatal pyæmia has followed the use of the clamp and cautery; and I myself have met with one case, which I will briefly relate.

A few years ago I operated on a gentleman, æt. 50, and removed, with the clamp and cautery, three large internal piles. On the fifth day after the operation the patient was out of bed, and appeared to be progressing in every way favourably. On the sixth day he had a rigor. On the seventh day he complained of a pain in his side, and symptoms of pneumonia

were present. On the tenth day he died, and evidently from acute pyæmia.

Although, therefore, acute pyæmia may follow this operation, I am strongly of opinion that there is less risk of its resulting from the use of the clamp and cautery than from the employment of the ligature. In confirmation of this, I think I am justified in stating, that experience has shown that a wound made—especially in vascular textures—by a heated wire, knife, or other instrument, in operative surgery, is attended with less risk of pyæmia and septicæmia than one made by other means, provided antiseptics are not employed—and the rectum is a situation where they cannot be satisfactorily used.

If the clamp and cautery are used for the removal of internal piles, it is very important that the cautery or other heated instrument should be carefully applied, and at an almost black heat. I have recently employed the thermo-cautery knife in two cases to cut off the piles after they have been seized with the clamp, and I have found it most simple and efficient in its application.

As is well known, internal piles are often complicated with external piles, or with a looseness or redundancy of the skin round the anus,—and it becomes a point of considerable practical importance to consider how far such complications should be dealt with when operating upon the internal tumours. When distinct external piles exist along with internal ones,—there can be no doubt that the proper practice is to cut them off at the time of operating upon the internal tumours; but, when the condition is simply a general looseness of the skin surrounding the anus, then I think that it should not be interfered with, unless it is very marked. I have seen very troublesome results from the too free removal of such skin, which, when the internal piles are protruded, seem more redundant than it really is. The plan I myself follow is to carefully examine the external parts after the internal piles and any prolapsed mucous membrane have been thoroughly pushed up into the rectum. If then well-marked external piles or any very redundant folds of skin are present, I consider it a proper case in which to cut them off; but if the looseness or folds of skin are not aggravated, it is better not to interfere with them.

In conclusion, and as a result of my experience, supported by the facts detailed, I would offer the following opinion in regard to the ligature *versus* the clamp and cautery:—That, although internal piles may be successfully removed by the ligature, their removal by the clamp and cautery is much to be preferred.—*Edinburgh Medical Journal*, *June* 1877, *p.* 1077.

ORGANS OF URINE AND GENERATION.

49.—ON URINARY CRYSTALS AND CALCULI; AND ON SOME OF THE CONDITIONS UNDER WHICH RENAL AND VESICAL CALCULI ARE PRODUCED.

By Dr. William Miller Ord, Senior Assistant-Physician and Lecturer on Physiology at St. Thomas's Hospital.

To Mr. George Rainey, my former teacher and deeply respected friend, are owed the inspirations which have led to this and similar investigations. His perfectly original observations on molecular coalescence constitute, as I believe, a discovery the value of which will be abundantly proved in time to come. In the present memoir I record some results of a pathological extension of his physical and developmental researches.

The starting-point of the observations and inferences recorded in this paper is that uric acid assumes remarkably different forms accordingly as it is deposited pure from solution in distilled water on the one hand, or coloured and impure from urine on the other. I beg, leave, therefore, in the outset to remind the Society of the general nature of this difference.

If some of the colourless or nearly colourless uric acid prepared from guano or serpent's urine be boiled in distilled water, and, after filtration, be left to crystallise from solution, the acid will be found in tabular crystals. Some of the tablets will be oblong in outline, and about six times as long as thick; such tablets, being perfectly transparent and homogeneous, may be compared to the oblong pieces of glass sold as paper-weights: they are the "oblong-square plates" of Dr. Thudichum. Others will be much thinner, much longer, six-sided in outline, with acute points, and may be compared to the common form of glass hand-plates on doors. There will be a number of intermediate forms connecting these extremes. It will be important to notice that the tablets remain generally separate and distinct from one another, or, if here and there aggregated, do not form regular star-shaped masses of crystals radiating from a common centre.

Uric acid deposited from urine differs from the pure uric acid in colour, form, and aggregation.

The crystals are always of a strong yellow or brownish colour, and are most commonly rhombohedra or modifications of the rhombohedron in which that form is easily made out. Prout ("Stomach and Urinary Diseases," 1840) gives two figures of uric acid. "The first represents," to use his words, "the ordinary appearance of lithic acid crystals under the microscope. The crystals are rhomboidal prisms, thin, transparent, and of a yellow colour. The second represents rhomboidal prisms of lithic acid, thicker than those depicted in the preceding figure,

and, in some instances, approaching to the cubic form. The cubic is much more rare than the rhomboidal variety of crystal." There is here no mention of the tabular forms. Dr. Beale, in his well-known work on "Urinary Deposits," describes or figures more than thirty forms of uric acid. The edition of 1869 does not contain one drawing of the perfect oblong tablets and only one of hexagonal crystals. A descriptive note beneath the latter points out that this form occurs in urine very rarely.

There must exist, therefore, in urine causes leading to a complete turning away of uric acid from its pure crystalline form. The change, too, is in a definite direction. The faces and angles of a crystal of urinary uric acid are almost always curved and rounded. Such a crystal is clearly the resultant of the operation of two distinct influences,—of crystalline polarity, under which flat surfaces and sharp angles are determined, and of molecular coalescence, in which polarity is lost and particles are arranged in spherical masses by virtue of their unhindered mutual attractions. Furthermore, in urine the crystals are very frequently gathered into larger glomeruli, to which such names as "gravel" and "cayenne pepper grains" are given. These, on examination, are found to be regularly constructed of radiating rhombs or prisms. The difference, therefore, between the *urinary* acid and the *pure* is very pronounced. So also is the difference among the forms of urinary acid. From Prout's two figures the record has gone on growing till as many as forty or fifty distinct varieties can be reckoned. Dr. Miller, Dr. Beale, and other writers on micrology and chemistry have suggested that the substances associated with uric acid in urine are the determining causes of the several differences; and the original observations of Mr. Rainey on the influence of colloids upon crystalline form—observations recently confirmed in a remarkable way by Harting's independent researches—have pointed the way to a solution of the problem. In the "St. Thomas's Hospital Reports" for the year 1870 I have published an account of a number of experiments and observations which led me to the following general conclusions:

1. That, in the presence of albumen and other colloids, uric acid was deposited in small, thick, subcubical crystals with much-curved faces, or in some kind of dumb-bell, or in some kind of spheroid. The forms plainly tended to sphericity.

2. That, in the presence of grape sugar and other crystalloids, the acid was deposited in large tabulate or foliaceous crystals with flat sides and sharp edges.

The first experiments were made with artificial solutions from which, in each case, everything but pure acid and one associate substance was excluded; but at the end of the paper a few drawings were given of forms of uric acid taken from albu-

minous and sugary urine respectively. The figures confirmed in a general way the principle obtained in the exclusive experiments, but left evidently much to be explained.

The present communication relates the results of further investigations, affecting not only uric acid, but also urate of ammonia, oxalate of lime, and triple phosphate.

Firstly. It appeared to me that one of the first subjects of inquiry should be the cause or causes of the difference between the pure and the urinary form of uric acid. Four groups of substances to which determining influence might probably be referred were to be found in urine,—mucus, urea and extractive, colouring matters, and salts. Mucus among these seemed most likely to mould uric acid. Mucus is a colloid, prone to decomposition, and active in promoting the decomposition of organic substances associated with it in solution. On the other hand, mucus in its fresh state is not actually held in solution in the urine, but merely suspended in a very much gelatinised condition. The first experiment consisted in removing the mucus from urine and observing the crystals of uric acid deposited after such removal. Absolute alcohol was added in equal bulk to fresh urine, of specific gravity 1020, clear, of a light sherry colour, and free from sugar or albumen. After standing some time the mixture was filtered, the mucus and some of the salts being thus removed from the liquid, which was afterwards heated to expel the alcohol. After all the alcohol was driven off, the remaining liquid was diluted with distilled water till the specific gravity of 1020 was restored. The liquid was next divided into four portions. To one a few drops of hydrochloric acid were added; in a second a few grains of pure uric acid were dissolved by the aid of heat; in a third and fourth washed uric acid from urine was dissolved, with and without the addition of two drops of glacial acetic acid. The four solutions were clear, and possessed the characteristic colour and smell of urine. Crystals were obtained from all four; they were in each case small rhombohedra with rounded angles, very much like those represented in Prout's first figure. To complete the experiment, the precipitate of mucus, &c., remaining upon the filter was washed with distilled water till the filtrate was colourless. The precipitate was then washed off the filter-paper and digested in three ounces of distilled water with three drops of hydrochloric acid; one ounce of a solution of pure uric acid in boiling water was added. The crystals deposited from this mixture were small; they were partly thick rhombohedra and partly short four-sided prisms with pointed ends, having therefore often a six-sided outline. They were not aggregated in any way, and were not adherent to the vessel.

The removal of the mucus from the urine, therefore, did not decidedly affect the form of the individual crystals. But their mode of deposit was different from the mode of deposit of uric acid from natural urine. They were caked on the sides of the vessel, and had to be detached for examination, and they were not in spherical radiating masses. Uric acid deposited from urine is rarely caked in this way, and is only in part attached to the side of the vessel. It usually lies in a loose powder among the mucus at the bottom of the vessel, and it is most commonly in radiating masses. The mucus probably arrests, by mechanical opposition, the fall of the crystals as they are formed in the solution, and places them in a favourable position for the addition of fresh crystalline matter on all sides of them. The spherical form of the crystalline masses indicates, I believe, more than a mechanical influence; indicates the molecule-disturbing influence of a colloid. The complementary experiment proves that the mucus does, in fact, affect the form of the crystals, and that the affection is like that of other colloids. But the necessary processes of the experiment cook the mucus, and tend decidedly to weaken its power. It is no more the same mucus that works in natural urine than boiled salt beef is living flesh. It is less putrefiable than fresh mucus, and has therefore less energy in transformation. If, thus weakened, it is capable of transforming uric acid, its natural influence must be considerable.

Supposing the uric acid of human urine and the uric acid having another source to be the same substance, the experiments showed conclusively that mucus with earthy phosphates, &c., was capable of impressing upon it the rhombohedral form, and that urine deprived of its mucus and earthy phosphates had the same power. It was necessary to pass to further exclusions.

Uric acid spontaneously deposited from urine was separated from the other constituents of urine by decantation and repeated washings. It was then thrown upon a filter and washed with distilled water containing a little acetic acid. There would remain then on the filter the uric acid, with perhaps a trace of mucus and epithelium. The prevailing forms of crystal in the uric acid used are drawn in fig. 2. [See illustrations accompanying Dr. Ord's paper.] Boiling distilled water was poured upon the filter, and the filtrate was collected and cooled. The solution was of a golden sherry colour, and yielded a large crop of yellow crystals.

Four further recrystallizations were made, each time with loss of colour, and the forms accompanying each loss of colour were noted and drawn (figs. 3, 4, 5 and 6). In the last all colour had disappeared, and the tabular forms of the pure acid were obtained in great beauty. (Uric acid and urates are notable

among urinary deposits for carrying down with them the colouring matter. They seem to take the dye much as animal fibres take dyes; and uric acid at least appears to be changed in its crystalline polarities by the dye in a degree corresponding to the amount of dye present with it in solution). Separate experiments were afterwards made with the various salts of urine. The influence of urea was known before to be in favour of the tablets, and the salts made little or no difference either way. The possibility of some of the constituents of the extractive combining with uric acid and modifying its form had occurred to me; but the experiments noted, and the fact that the form of the acid is the same when deposited in the presence of considerable excess of other acids, were against the supposition.

The final conclusion was that mucus and colouring matter were certainly both agents in determining the ordinary urinary form of uric acid, and that they were probably the sole agents.

Forms of uric acid in Albuminous Urine.—These are always different from the forms found in ordinary urine, but they differ remarkably among themselves. They are small, rounded, and compact, presenting many gradations from the subcubical crystal with curved sides, such as is drawn in Prout's second figure, to the sphere. I have not been able to trace any connection between the variations of form and variations in the quantity of albumen present. Neither have I any observations recording the influence of isomeric varieties of albumen. Urea, however, does appear to be one among the causes of variation. Urea alone, in solutions of specific gravity less than 1040, promotes the formation of large, very thin plates of uric acid, and numerous observations indicate that a large proportionate quantity of urea in albuminous urine constitutes *pro ratâ* an obstacle to the sphere-forming process.

Forms of uric acid in Sugary Urine.—Here the crystals tend to be tabular. Whereas with albumen the acute angles grow larger and the obtuse angles smaller till equality is attained and the cube prevails, with sugar the obtuseness of obtuse angles and the acuteness of acute angles are increased; whereas with albumen the axis joining the flat sides of the rhombohedra is lengthened, with sugar that axis is shortened. Two forms of uric acid may be noted in connection with sugar in urine; one oblong or square in outline, with fine chiselled ends and thicker middle; the other much less common, with flat parallel surfaces and diamond-shaped or hexagonal outline.

Forms of uric acid in Purulent Mucus.—Colloidal substances allied in composition or reactions to purulent mucus contribute, if my observations are correct, to the formation of stone in the kidney and bladder. The urine of chronic cystitis, abounding in purulent mucus, is mostly advanced in decomposition far

enough to have become ammoniacal, and filled with precipitated phosphates at the time when examination has been possible. Urine of this kind, secured early, and, after gentle warning, treated with a few drops of hydrochloric acid, gives very interesting forms. In the fluid portion of such urine rhombs and rosettes are formed just as in natural urine. In the mucus, cubes and rounded bodies and dense stalactitic agglomerations of uric acid are deposited, the precipitate within the mucus being much larger than in the surrounding fluid. Artificial conjunctions show very clearly that the actively changing colloids entering into the matter of purulent mucus exert a strong converting and cementing influence on uric acid.

Forms of Urates.—In many cases I have observed small brown spherules, soluble at moderate temperature, in albuminous urine depositing uric acid. They dissolved much more readily than the acid, and were either urate of soda or urate of ammonia.

In ammoniacal urine containing much purulent mucus I have frequently seen urates in three forms: 1, perfect spheres; 2, spheres with regular radiating raphides; 3, spheres with an investment of branched and irregular or bristly raphides ("hedge-hog" crystals). I have procured these in great abundance by adding to an acid urine containing large quantities of urate of ammonia the stringy mucus from ammoniacal urine of chronic cystitis. The mixture being first warmed to dissolve the urates, they are found, after cooling, deposited in two forms; within the mucus as spheres and hedge-hog crystals, elsewhere as granules. I have also often observed the spherules of urate of ammonia which have been described by Dr. Roberts as formed in decomposing urine, and have no doubt of their being formed under the influence of the chemically-active mucus.

A spherical form of urate of soda was figured, at first under the name of urate of ammonia, by Dr. Golding Bird in his urinary deposits. He described it as follows:—"Spherules of urate of ammonia with crystals of uric acid adhering." And he stated that this form was occasionally observed in albuminous urine occurring in scarlatina. Dr. Thudichum, Dr. Beale, Dr. Roberts, and Drs. Ultzmann and Hofmann, have since figured similar forms. There is no further note of albumen, but the associated conditions were usually febrile attacks in children. Drs. Thudichum and Roberts have added a third condition, evidently of much importance—delay of the urine in the urinary passages. In all the cases where any history or explanation is given there have been states favouring the presence of colloids, either of albumen or of mucus, or of other results of rapid febrile action. And delay in the urinary passages, keeping the urates long in contact with putrescent colloids, at a temperature

most probably above 100° Fahr., must have added a very strong impulse to the forces determining spherical form.

The urates, though the most common of all deposits from cooled urine, enter much less frequently than uric acid into the composition of calculi. Their comparative solubility, and their habit of assuming a fine molecular (amorphous ?) state without running into masses, will account for this. Their solubility has been shown by Dr. Bence Jones to be increased by the presence of chloride of sodium, distilled water being rendered capable of taking up double the quantity of urate of ammonia when the chloride is added in small quantity. The same addition prevents urate of ammonia from taking the crystalline form. Dr. Bence Jones states, that if urine is added to a warm solution of needles of urate of ammonia in such small quantity as not to cause any discoloration of the subsequent deposit, that deposit is always amorphous. He traces this to the chloride of sodium. Whether the colouring matter, which adheres with such tenacity to the urates that it is best obtained from them (Golding Bird, Thudichum), affects the form of urates, I have not as yet determined.

Oxalate of lime.—Dr. Beale has figured dumb-bells of oxalate of lime in casts of urinary tubules. His remarks upon these figures, indeed, directed my thoughts to the present investigation. Following up Dr. Beale's hints, I have shown elsewhere that oxalate of lime is much modified in its crystalline form by the presence of colloids, exchanging the octohedron for various tablets, dumb-bells, "wheat-sheaves," and spheres.

Influence of Albumen on Nitrate of Urea.—Nitrate of urea, as is well known, can often be obtained from urine without previous concentration. The crystals thrown down from urine of a specific gravity exceeding 1037 after the addition of an equal bulk of nitric acid are shining flakes, often cohering in irregular masses and distinctly darker than the urine. If urine of such specific gravity contain a moderate quantity of albumen, the addition of an equal bulk of nitric acid causes the solution of the first precipitate, and a perfectly clear liquid is obtained. The process of crystallization is in this case very different. Instead of dark flakes, tiny globes of a light colour are suddenly seen suspended at several points in the liquid and grow rapidly under the eye. They are composed of fine radiating acicular crystals, and look like little drops of fat. That albumen is the cause of this growth of spheres may be readily proved. Another portion of the fresh urine is freed from albumen by boiling, the addition of two drops of nitric acid, and filtration. The filtrate, treated as before with nitric acid, yields flaky crystals.

Bearing of the foregoing observations on Microscopic Morphology.—Dr. Carter in his remarks on the formation of calculi does

not fail to recognise the importance of the higher temperature of the body as a condition of the process. How far this influence is to be attributed to the unchanged movements of heat, and how far to induced molecular changes in the colloids, cannot be decided or calculated at present. Both of these will, of course, come under the head of molecular disturbances, but whether one or other form of molecular movement is more apt to blur the lines of crystallization I do not pretend to suggest. It is only necessary now to point out that heat plays a remarkable part in determining the assumption of spheroidal shape by crystalloids in the presence of colloids. I have already, in the paper referred to, shown this to be the case when oxalate of lime is deposited in albumen. Experiments with phosphate of lime and carbonate of lime in albumen have since confirmed the first inductions.

I have long felt that the present inquiry is only part of a much larger questioning of nature—of an inquiry into the causes, general and particular, by which the morphology of tissues is determined. Microscopists have been content for the most part to record the forms of the tissues with constantly increasing research and accuracy. This is undoubtedly a work of importance, indeed of necessity, but is, to my mind, only a preliminary process, bearing to the higher morphology of the future the sort of relation borne by the Linnæan to the Natural system in Botany. There must follow a natural system, intrepreting the meanings of forms, compelling form to tell the story of growth and function, recording the relations between form and substance, and proceeding to the discovery of the laws of tissue formation. The line of thought into which my observations have directed me leads to the belief that such further research will put us in possession of new methods in diagnosis and therapeutics.—*Medico-Chirurgical Transactions, vol.* lviii., *p.* 165.

50.—ON LITHOLYSIS.

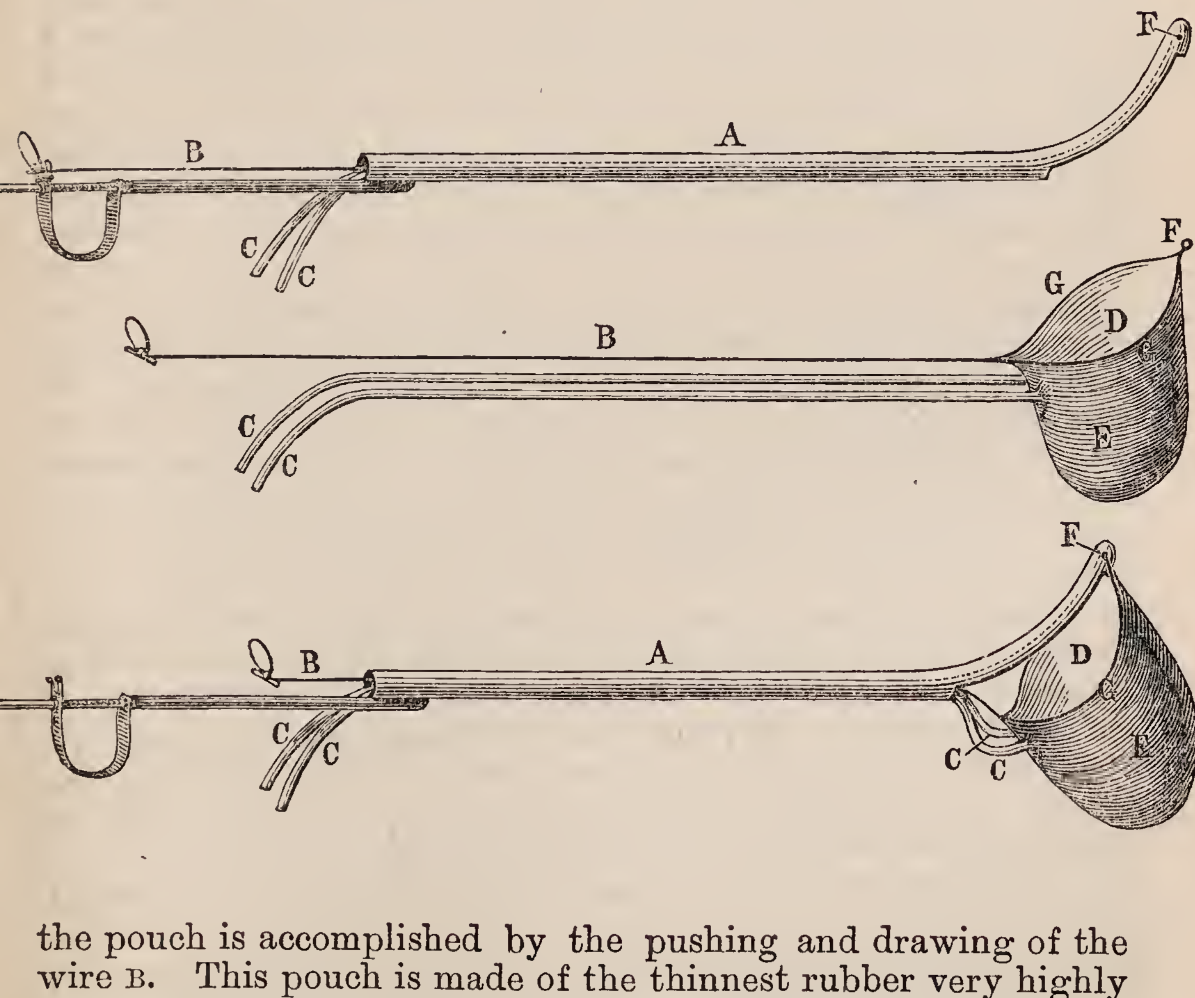
By Dr. George C. Duncan.

[By litholysis is meant solution of a calculus. Dr. Duncan in describing his instrument for the treatment of stone by this method, says, "I do not pretend to say that all urinary calculi can be got rid of by litholysis, but that some cases may be successfully operated upon; and, if the percentage of these be ever so small, it is, I think, worthy to be placed in the list of means for their removal."]

In a former article (*Retrospect*, vol. 74, p. xxxviii.,) I stated that the method proposed was to encase the calculus in a thin pouch of indiarubber, introduced by a suitable contrivance, which is somewhat similar to an ordinary catheter, the difference

being that about one-third of the convex side of the curved part is wanting, making a sort of trough in which lies the rubber pouch when empty, and in readiness for either insertion or extraction; and entering this pouch are two rubber tubes which communicate from it through the catheter to the operator.

The instrument consists, in the first place, of an ordinary catheter A, gold-plated to prevent any corrosive action taking place from any of the acids used coming in contact with it. Running through this catheter is a small stiff steel wire B, attached to one end of an elliptical steel spring G, which is fixed at the other end to the point of the catheter, where it works on the hinged joint F, so that when the wire B is forced through the catheter, the spring which is fixed at the joint F is allowed to open, and, by drawing the wire B in the opposite direction, the spring is forced to close and lie in the trough of the catheter. Attached to this spring G is rubber pouch E, the mouth D of which is surrounded by the spring G, so that the opening and closing of



the pouch is accomplished by the pushing and drawing of the wire B. This pouch is made of the thinnest rubber very highly vulcanized, ensuring it of elasticity and strength, yet taking up no more room than the trough of the catheter can easily afford.

The size of this pouch is about two inches in depth, and the diameter about the same size as the spring, which is about two and a half inches in its long diameter, and one and three-quarters in its short, when open to its full capacity. The wire attached to this spring (which in other words is the mouth of the pouch), when withdrawn, is securely fastened to a contrivance of which no other description need be given than that it is arranged with a screw by which any amount of traction can be applied to the wire B, which ensures a safe closure of the pouch E, as the curvature of thecatheter serves, as it were, for its *point d'appui*. Entering this pouch at the side next the operator are two small rubber tubes CC, their entrance being close at the edge, just allowing free communication with the interior when it is closed; these, as before stated, pass through the interior of the catheter to the operator. As will be seen by the above, the mouth of the pouch must be closed, and the bulk of it placed in the hollow of the catheter before introducing it into the bladder; if, then, by using the catheter as a sound, the situation of the stone can easily be ascertained, which, being done, pass the instrument to one side, so that the stone will be on the other side of the instrument to that on which lies the pendant part of the pouch, or, in other words, in apposition to the mouth of the pouch; then, by forcing the wire or stilet through the catheter, this mouth is opened ready for the entrance of the stone, which can be accomplished by making a lateral movement with the outer end of the catheter. It can be ascertained when in the entrance by drawing on the wire, which, of course, cannot be withdrawn, if there, to its usual distance. Having arrived at this stage, it will be found to be the safest plan to firmly hold the stone in the entrance until, by pressing the catheter to one side, the mouth of the pouch is nearly in a horizontal position; then, by withdrawing the traction from the wire, the spring opens of its own accord and allows the stone to drop into the pouch, which, being accomplished, the wire is drawn out and fastened to the apparatus, by which a sufficient amount of traction can be given to ensure a complete closure of the entrance of the pouch. The stone, being secured in the pouch, perfectly isolates it from the walls of the bladder, so that the next step in the the operation is the introduction of the solvent, which is done by a graduated glass syringe, the nozzle of which fits into the end of one of the tubes. It may be as well to mention here, that the object for the syringe being graduated is to see that the same amount of fluid escapes from the exit tube that is forced into the pouch, for fear that some of the solution might escape into the bladder owing to some imperfection in the closure of the entrance.

In theory, different solvents will be required for the solution

of the different formations; but, practically, nitric acid will, I think, be the solvent; I might say, *par excellos*, even the cholesterine formation is said be soluble in it as well as in alcohol. No doubt, it will be at once asked, Will not the acid destroy the pouch and tubes, and in what strength will it be used?

Experience has taught me that the rubber is not acted upon by the acid, and the strength will be according to the length of time wished for the operation, and the size of the stone, with the general state of the patient. A solution containing about fifty per cent. of acid disintegrates them very rapidly. The operator must be governed in the strength of his solution chiefly by the formation, of which the microscope must be his guide. I shall have to defer until another time some experiments concerning the different degrees of solubility of the different formation of vesical calculi, which I wished to have given here, on account of having been disappointed in the receipt of some specimens promised. It may also naturally be asked, in case of any rupture or imperfect closure of the pouch, what will then be the consequence, and what must be done? By having a graduated gauge on both the tube of entrance and exit, it can easily be ascertained when the same quantity does not escape that has been injected, and if such an accident should happen, by having taking the precaution before introducing the instrument to have the bladder partly filled with urine, so that the small amount of acid that might escape will be diluted to such a degree that no harm to the bladder will result. As a precaution, it might be well to have an extra syringe and an alkaline solution at hand, so that it could be injected through the catheter into the bladder, thus thoroughly preventing any further trouble. My reason for mentioning this is, that an objection has been frequently made to me while discussing the feasibility of the plan, which was, that the formation of gas from the disintegrating stone might, if the tubes became obstructed, burst the pouch and allow the acid to escape into the bladder. Allowing that such an accident should occur, and taking into consideration the small amount of acid in the pouch, and the neutralising effect of the stone, together with the dilution it would receive from the urine, and the injection of the fluid from without, I do not think that any serious harm would result.

Electricity has been proposed to disintegrate stone; but I have had no experience with it as yet. It has occurred to me, if it is of use, that a circuit could be made by passing wires through the tubes instead of fluid, so that the stone would be between the poles of the battery.—*Edinburgh Medical Journal, May* 1877, *p.* 995.

51.—ON LITHOTOMY BY THE RECTANGULAR STAFF.

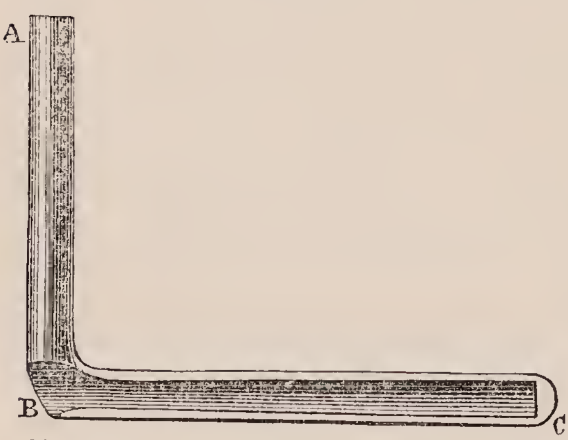
By George H. B. Macleod, Esq., F.R.S.E., Regius Professor of Surgery, University of Glasgow; and Lecturer on Clinical Surgery, Western Infirmary.

[In the year 1848, Dr. Buchanan, of Glasgow, published his paper explanatory of the operation by means of the rectangular staff (*Retrospect*, vol. xvii., Jan. to June, 1848, p. 214). This operation has however never come into use in other places, notwithstanding its obvious advantages.]

In Glasgow the rectangular staff is almost exclusively employed, and confidence in it has steadily increased from year to year, while I do not think it has ever been fairly tried elsewhere, unless I except its occasional use by Mr. Hutchinson in London. I cannot but hope that if its merits were better known it would be adopted by the profession in general throughout the country.

In the original paper the author tells us how he was led by a perusal of Dupuytren's "Memoir on Lithotomy" to institute experiments, and how gradually he came to fashion his new staff, and adopt the "improved method of penetrating into the bladder, and dividing the more external parts." The staff used by Dr. Buchanan is bent at right angles three inches from the point, and is hence "rectangular." It has a lateral groove along the horizontal part (B, C, Fig. 1), and the end of this groove is closed at C. When the instrument is introduced, the angle B lies in the membranous part of the urethra, close in front of the prostate gland, and can be felt by the finger placed in the rectum, or by a little pressure on the perineum, to occupy a point a little in front of the anus. The horizontal part lies parallel to the rectum, and extends into the bladder.

Fig. 1.



In operating, the staff is so held as to occupy an intermediate position between being hooked up under the pubis and being pressed down on the perineum, and the operator keeps it steady and distinguishes its correct position by placing his left forefinger in the rectum under its horizontal part. The thumb of the left hand is at the same time pressed gently in front of the anus, so as to mark the site of the angle and to keep it steady. The exact position of the angle is very easily determined, as there are only the skin, superficial fascia, and some fibres of the

sphincter between it and the thumb. The knife used is a straight-backed one, whose blade exceeds in length the grooved portion of the staff by about one-fourth of an inch. The point is sharp, and it should have a cutting edge on its back for about half its length, by which the tissues along the groove are more surely divided towards the middle line of the perineum. The shoulder of the knife is low, and the breadth of the blade equal from shoulder to hilt.

When the patient has been tied in the usual position, and the staff placed as above described and fixed by the operator's left hand, the knife (held short and above the hand, palm upwards) is slowly inserted close above the anus, "just where the mucous membrane shades into skin," and close to the raphé. The edge is turned to the left side of the perineum, or to the operator's right. The blade is not introduced parallel to the horizontal part of the staff (which would greatly increase the risk of its escaping from the groove as it passed on into the bladder), but obliquely, so as to impinge on the groove at an angle; and as it is afterwards pushed on towards the bladder, a *slight* obliquity is still maintained, so as to assure the operator that the point is in the groove, and to ensure its non-escape therefrom. In this way the whole length of the groove is traversed, and the point of the blade finally arrested by the closed end of the staff. It is then best slightly to withdraw the blade and to complete the division of the soft parts as it is brought out, the knife being "lateralised" and made to cut in a semi-circular direction between the anus and the tuberosity to a point rather behind the level of the anus. This whole cut may measure from $1\frac{1}{8}$ in. to $1\frac{1}{2}$ in. according to the development of the parts. "It approaches," says Dr. Buchanan, "very nearly to one-half of Dupuytren's incision, only it lies much nearer the rectum, and, though little different in size, involves a larger portion of the circumference of the intestine." Nothing now remains but to insert the left forefinger, following the horizontal portion of the staff into the bladder, and dilate the very limited wound found in the prostate, while the staff is withdrawn and the stone extracted in the usual way.

It will be observed that no incision is made over the angle of the staff before it is penetrated. To make such a preliminary cut only complicates matters. No aid is got from it, and the parallelism of the two cuts is difficult to ensure. The staff is opened at one thrust, and if the precautions above described are taken there is no danger in this step. The knife should never be pushed on till the operator feels confident that it is in the groove. This the grating of the point of the knife on the groove makes very evident to him, and the assistant holding the staff also plainly perceives it.

From the above description it will be apparent—(1) That in this method of operating, only one incision or cut is, as a rule, required, and no dissection called for. (2) That the incision lies lower down (*i.e.*, nearer the anus) than in Cheselden's operation. (3) That the urethra is opened considerably nearer the bladder than it is in the lateral operation. (4) That a straight, short, and direct road is followed to the bladder; the prostate gland being reached at once at the point where it most nearly approaches the surface of the perineum. (5) That less injury is done to the soft parts of the perineum and the urethra than in the ordinary operation, the incision, though all that is necessary for the purpose in view, being much shorter and more limited. (6) That there is much less danger of wounding important bloodvessels, as the incision does not go near them. (7) That the rectum is, by the action of the horizontal portion of the staff, rendered straight, and is therefore not in the least danger of being wounded, as at first might be supposed it would be. (8) From the near neighbourhood of the anus to the incision the wound is easily stretched or dilated, so that it does not require to be of great size. (9) A more limited incision is made in the neck of the bladder than is usually inflicted in the lateral operation, and the wound lies in the longest axis of the prostate. (10) If the stone is very large and much room is needed, the right side of the prostate is easily reached, and can be incised with a probe-pointed bistoury.

In short, I hold that this mode of operating most perfectly fulfils all the requirements of an easy, rapid, and safe access into the bladder; that the surgeon cannot go wrong who exercises the most trifling care; that there is the least injury to structures and the minimum risk of complications; that it provides the shortest road for the stone to travel as it is extracted, and that the most direct and efficient drain for the urine is established.

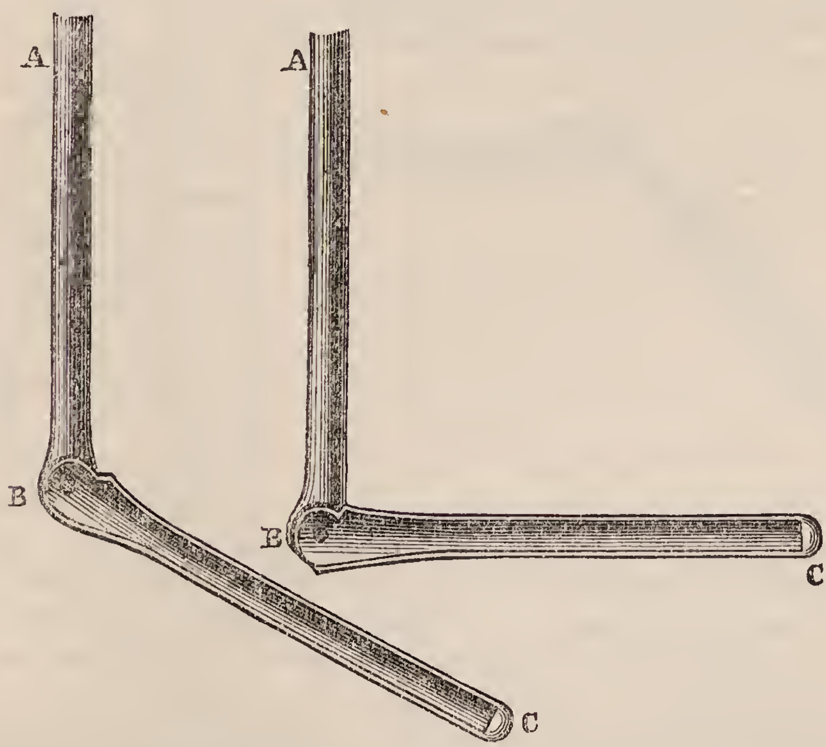
In speaking to hospital surgeons elsewhere of this operation, I have always found that their objections to use it were either, (1) the supposed difficulty of introducing (especially in children) a staff of the rectangular form; (2) "the stab in the dark," as the passing of the knife into the angle was occasionally termed; (3) the risk of the knife escaping from the groove; and, lastly, the supposed danger of wounding the rectum.

There is no doubt but that the first objection is well founded. It requires care to pass the heel of the staff especially through the meatus, even though the operator catches the staff short (as he should do), and exercises every care. So too, in watching the heel, beginners are apt to get the point caught at the subpubic curve, and it was to overcome this difficulty that I had the staff changed, as afterwards described. The second objection has not

much practical force. The heel is so easily defined by the fore-finger and thumb of the left hand applied in the way above indicated, that it is readily entered. Yet some operators hesitate to attempt this by one movement of the knife, and make a preliminary incision to that by which the point of the blade is placed in the groove. This should, however, be avoided, as was before explained. The third objection is groundless if the rule I gave is followed—viz., to insert the knife at an angle into the groove, and to keep it at a slight angle to the horizontal portion of the staff all the way into the bladder. In this way the groove is "felt" all the way by the point of the knife. The fourth objection is quite untenable. It is suggested by experience of the curved staff, which, from its shape and the way it is held in lithotomy, exaggerates the curvature of the bowel. The rectangular staff, on the contrary, renders the upper surface of the rectum straight, and no injury whatever to the bowel attends an operation performed with it. There is a tradition in the school that it was once wounded, but I have never known it happen in my day.

It was however, to overcome the force of the first and second objections that I was led to alter the construction of the rectangular staff. After trying various expedients, I have finally had the staff hinged by a very simple and effective mechanism, ably executed by Mr. Hilliard, of this city (see Fig.

Fig. 2.

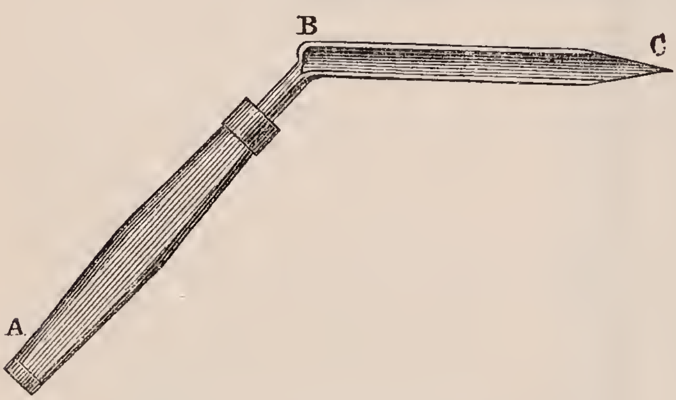


2), so that when being introduced it can be placed in the most favourable position for being passed along the canal, and when

it is in place, by turning a screw in the handle, it is firmly fixed in a rectangular position. The pressure of the left forefinger in the rectum brings it to its right-angled position (and that it cannot pass), and then two turns of the screw fixes it there. By this simple plan much is gained. First, all difficulty of introducing it is overcome. Secondly, the heel of the staff (which is the point we seek for, and which there may be a difficulty in finding if the staff is a small one), may be greatly enlarged (widened and made more easily detected), and so more surely entered. And, thirdly, the removal of the staff from the urethra is also facilitated, as by reversing the screw the horizontal portion is allowed to fall, and so the angular shape of the staff done away with. So long as the staff was rigid at the angle the heel had to be kept small, as it was in passing it through the meatus, and in conducting it and the point along the canal that the difficulty lay, but with the hinge at the angle a large wide heel can be easily passed, and so made available. I have used this new instrument four times on the living with great satisfaction and the hearty approbation of those who have seen its simplicity of action.

In order to overcome the distrust some have of passing the knife at once into the groove, I some time ago tried a plan of inserting from the perineum, at the point where the knife is to enter, a sharp steel director set in a handle, as shown in Fig. 3. The groove in the staff was so made that this director fitted accurately into it, and when pushed home got fixed into the groove, The director was introduced just as it was before described the knife should be—viz., a little obliquely to the horizontal part of the staff, and so soon as its point was felt to be in the groove it was pushed on parallel to the groove till it fixed itself in it. A tiny stream of urine came along the groove in the director, showing at once that a road to the bladder had been secured, and the knife was then run along the combined grooves of the director and staff, and the operation completed in the usual way. One advantage of this plan was the perfect firmness with which the staff was fixed and held in place by the director, and the great prominence you could give to the angle of the staff in the perineum before the director was inserted, as so soon as it was in place you could adjust the horizontal part

FIG. 3.

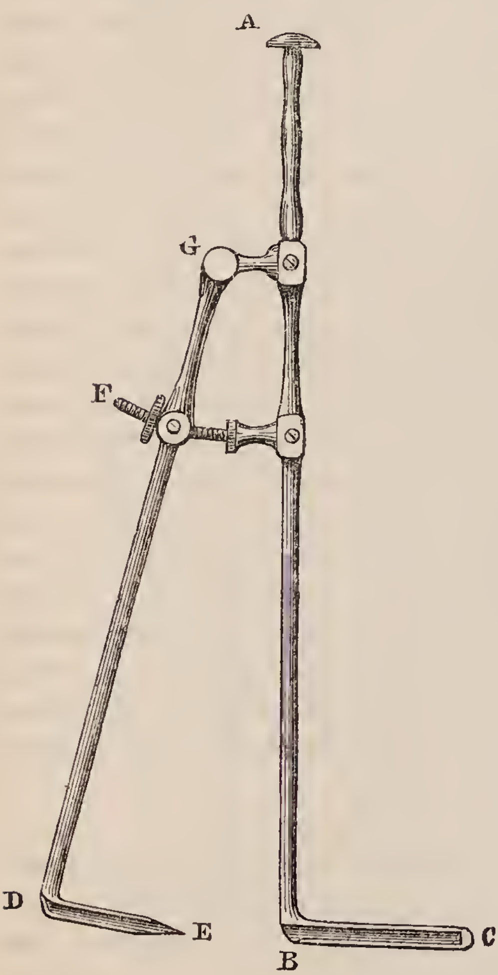


of the staff, and fix it as you pleased. I operated several times on the dead subject in this way, and once on the living. It was quite successful, but too mechanical for common use, and, as I believe that all difficulty of finding the heel of the staff is overcome by the device I have related in this paper, I have not pursued the practice of using the director now mentioned.

It is well known that Mr. Earle contrived an outside guide to the ordinary lithotomy staff, and the late Dr. Corbett tried a similar arrangement for the rectangular one. His instrument is depicted in Fig. 4. Its adjustment was troublesome and not always accurate, and its application so purely mechanical that it never came into use; but if carefully applied, it secures the passage into the bladder, in a manner so obvious and certain, that the merest novice may pursue it, so far as the mere incision from the surface is concerned. The sharp-pointed (E) outside staff, when pushed in through the tissues of the perineum, at once fixed itself in the true staff (B) lodged in the urethra, and the knife was run along the united grooves (D to C) from the surface into the bladder.

I may further remark, in conclusion, that in this school there is a remarkably good and sure plan of catching the stone employed which I have never seen used elsewhere nor described by anyone. After the forceps have been passed into the bladder in the usual way, and the position of the stone ascertained, if it is lying, as is usual, on the floor of the bladder (and elevating the pelvis by placing a pillow under the hips greatly facilitates the manœuvre), the handles of the forceps are widely separated by the operator taking one in each hand, and then by a sudden sweeping movement he brings his right hand downwards and his left upwards, by which the right blade is with very great certainty passed under the stone, and it is finally grasped by approximating the hands. Every operator will acknowledge that lying hold of the stone is often the most troublesome part of the operation, and this I believe is in no small

Fig. 4.



measure due to the forceps being used, like scissors or sugar-tongs, with one hand, and not with both hands, as here stated.

I am under obligation to my friend and late house-surgeon, Dr. Moffat, for the drawings illustrating this paper.—*Lancet, April 28, 1877, p. 601.*

52.—ON INCISION OF HYDROCELE ANTISEPTICALLY AS A MEANS OF RADICAL CURE IN CERTAIN CASES.

By W. H. A. Jacobson, Esq., M.B., F.R.C.S., Assistant-Surgeon to Guy's Hospital.

[This method is not brought forward as one preferable in ordinary cases to that usually employed—viz., the injection of iodine.]

Given a patient with hydrocele, in whom this method is to be employed, the only preliminary required is to shave the pubes and scrotum on the affected side; an incision is then made with the ordinary antiseptic precautions over the upper part of the hydrocele downwards for a distance varying with its size, one of about two inches will usually be sufficient. This incision should go at once down to the tunica vaginalis, which is nearly always thickened, and readily recognised. A corresponding incision is then made through the tunica vaginalis, and this should be done quickly; otherwise, as the long-stretched dartos rapidly contracts, the resulting aperture will be found too small for the satisfactory carrying out of the next step. This consists in tucking in a narrow strip of gauze soaked in carbolic oil. The object of this is to ensure that granulations shall spring up all over the opened sac so as to procure its obliteration. The usual antiseptic dressings are then applied, overlapping the wound carefully in every direction, perforated for the passage of the penis, and kept in position by a double spica bandage. The wound should be dressed once in the two following days, after this every second, third, or fourth day. All that is required is to syringe out the wound with a solution of carbolic acid (1 in 40), and on the first one or two occasions to reinsert a fresh strip of oiled gauze. Four or five days after the operation the patient may get up, and at the end of a fortnight he may leave the hospital. If the wound has not completely healed by this time all that will be left will be a small granulating surface, and the only treatment needed is to reapply every other day a bit of boracic lint, and to keep the testicle suspended a little longer.

To proceed with the disadvantages of the operation:—

1. It involves more trouble undoubtedly than the treatment by injection. The operation itself may be completed in ten minutes, but an anæsthetic will generally be required, and there is the trouble of the subsequent dressings.

2. Another objection may be raised that this method must require a much longer time than that of injection. To a certain extent only is this objection a valid one. The five cases which I treated in this way had an average duration of treatment of just over three weeks. I find, from an analysis of forty-three cases in which iodine was injected, the average duration of treatment, or rather of their stay in the hospital, was quite fifteen days. But, as bearing upon this question of time, I find frequently at the end of the reports of these cases of patients who, after injection, left the hospital at the end of the second week, some such note as, "Scrotum as large as before, but painless," "Scrotum still twice natural size," &c. It is obvious therefore that a patient cannot do much work in this condition. Mr. Curling says, speaking of the cure by injection of iodine, "in about three weeks the cure is usually accomplished." If therefore three weeks and a few days be allowed as the time required for the accomplishing of the cure after the antiseptic incision, the difference is but slight.

3. It may be objected that such an operation as the one described above is severe, and involves risk. In answer to this it is only fair to remember that incising the tunica vaginalis with antiseptic precautions is totally different from the operation of incision as carried out a hundred years ago. For instance, Samuel Sharp (one of the earliest surgeons at Guy's Hospital) describes four cases treated by the old method of incision in his Treatise on the Operations of Surgery (published 1739). From the severe "symptomatick" or traumatic fever, and the smart secondary hemorrhage from the vessels of the inflamed scrotum, the suppuration and sloughing, he speaks of these cases "as a catalogue of misfortunes," and of himself as having been threatened with the death of his patients, though none actually died. Contrast this with the cases operated on by the antiseptic incision, the temperature in not one of them rising at any time higher than 99·8°, and that only in the thirty-six hours following the operation, then at once falling to normal, and remaining so throughout the rest of the treatment; any constitutional disturbance present during the first twenty-four hours being of the most trivial kind, and afterwards entirely absent.

I have spoken separately of the disadvantages of this method. In bringing this paper to an end I will consider together the advantages of the method and the cases to which it would appear to me to be specially applicable.

(*a*) It is absolutely certain, for the cavity of the tunica vaginalis is obliterated by granulations. On account of this advantage I should be inclined to use this method where attempts at a radical cure by injection with iodine have already failed. I

quite admit that this is rare, but it undoubtedly does occur. Out of forty-four cases of which I have notes, the injection with iodine failed in eight, and in two of these it failed twice. I may add that these cases were under the care of the surgeons and assistant-surgeons at Guy's Hospital, and that in some cases the injection used was of the ordinary strength, equal parts of water and the tincture; in others the undiluted tincture. I would also make use of this method, not only where injection has already failed, but also where there is a risk of its failing—*i.e.*, where the sac has very thickened walls, or where the hydrocele is very large in size; for here, too, the injection by iodine may fail, partly because all the fluid may not be removed by tapping, and also on account of the difficulty of bringing the iodine injected thoroughly in contact with the inner surface of the tunica vaginalis.

(*b*) This method may be safely employed in cases where a radical cure is desired, but where from ill-health or age injection with iodine may be thought to be contra-indicated. Thus one of the cases in which I used it was that of a dissipated London tavern-waiter much let down in health, and having a crop of boils about him, and a small carbuncle on his neck. The man was about fifty, and the hydrocele a large one, which had been tapped ten times. In this case I should not have liked to have employed the iodine injection, but I had no hesitation in employing the antiseptic incision, and the man was relieved of his troubles at one and the same time, for, while the tunica vaginalis was filling up, his neck healed.

(*c*) In some cases of congenital hydrocele which have resisted milder means, I believe that a careful incision, with antiseptic precautions, will obliterate the communication with the peritoneal cavity quite as safely and far more surely than the pressure of a truss, which is usually recommended to be perseveringly applied. [Since this paper was read, a case of congenital hydrocele in a boy aged seven has come under my care. Discutient lotions and blistering with iodine having had no effect, the sac was incised with antiseptic precautions, and the inguinal canal carefully plugged with a strip of carbolised gauze for forty-eight hours. The child was cured, having had no bad symptom from first to last.] It would appear to me that this method might also be found useful in certain other cases of hydrocele in children—viz., where the point of obliteration of the peritoneal cavity is high up, or, in other words, near to the peritoneal cavity.

(*d*) There are certain, though not very common, complications of hydrocele in which the surgeon may be desirous of exploring the cavity of the tunica vaginalis. Thus many will remember the case of hydrocele related by Sir B. Brodie, where

the patient, after each tapping, used to suffer excessive pain, "lying down on the floor and groaning with agony for a quarter of an hour." After death, a loose cartilage was found in the tunica vaginalis cavity. Prof. Humphry relates a somewhat similar case. These bodies may originate in one of two ways—(1) by some inflammatory material deposited in or beneath the tunica vaginalis gradually being pushed forwards, and, as in the knee-joint, becoming stalked and then detached; (2) and I should think more commonly, as pointed out by Mr. Osborn in vol. v. of St. Thomas's Hospital Reports, they originate in the hydatid of Morgagni. That this may be so was well shown in one of the cases which I treated by incision. In this case, after the fluid had escaped, the hydatid of Morgagni was seen in its usual place and of its usual size. In addition to this was another body with a long and very narrow peduncle. This might readily have become detached, and, already fibrous, on becoming solid might have been in its structure and behaviour much like a foreign body in a joint.

Another class of cases in which I think this method may be usefully employed to explore the cavity of the tunica vaginalis is where a hydrocele is complicated with an enlarged testis, the enlargement being of a doubtful nature. Thus a man aged about thirty lately came to me as an out-patient. He had a large hydrocele which had been tapped on two occasions; the testis was more than twice its natural size, hard, and somewhat irregular; the cord healthy. The hydrocele, and he thinks also the enlargement of the testis, date back about a year to a strain received in jumping out of a cart. There is no constitutional history whatever. He is anxious to have the hydrocele radically cured. I have delayed this to see the effect of another simple tapping, mercury internally, and strapping to the testis. If after a time the testis remain enlarged, I should consider the hydrocele not one for injection with iodine, but a case suited rather for laying open the tunica vaginalis, after the method which has been described above, so as to explore thoroughly the enlargement of the testis.

Again, in cases of vaginal hydrocele complicated with encysted hydrocele of the testis it may be useful to employ this method. Most surgeons know of cases where, after tapping a vaginal hydrocele, some swelling still remains, usually confined to the neighbourhood of the epididymis, and often yielding, as is well known, a different fluid from that let out from the tunica vaginalis. Tapping and injecting both the vaginal and encysted hydrocele will often result in a permanent cure; but, to my mind, laying open the hydrocele of the tunica vaginalis first, and then incising at the same time and with the same precautions the encysted hydrocele, rather recommends itself, the

more especially because the encysted hydrocele is not always single, in which case, if one be incised by the trocar and cannula, the cure will be incomplete—a risk not likely to be run if the hydrocele of the tunica vaginalis be first freely laid open so as to allow of the other being thoroughly explored.

Lastly, I will refer to certain cases of hydrocele complicated with hernia; and having already spoken of congenital hydrocele, I now only mean hydrocele complicated with hernia, in the adult. For the radical cure of such a hydrocele I should always make use of an incision with antiseptic precautions as a means of radical cure, in preference to injecting with iodine. It must be remembered that in these cases the hernia may be, and often is, irreducible; and in addition to the risk of puncturing the bowel with the injecting trocar and canula (a risk which may of course be avoided by care), there is another risk over which the surgeon has less control—namely, of the inflammation set up by the iodine extending to the sac of the hernia and so to the peritoneal cavity.—*Lancet*, *Sept*. 1, 1877, *p*. 309.

53.—ON THE TREATMENT OF HYDROCELE.

By Samuel Osborn, Esq., F.R.C.S., St. Thomas's Hospital.

The cure of a hydrocele is effected in one of two ways, either by the simple withdrawal of the excess of serum, with or without the injection of mild styptics, whereby a fresh and healthy action is imparted to the tunica vaginalis and secretion and absorption are carried on reciprocally; or by the obliteration of the sac, either by the injection of strong styptics, whereby adhesive inflammation is set up and the sides of the tunica vaginalis become more or less approximated, and adhesions take place between the visceral and parietal layers, or by allowing the sac to suppurate and granulate up from the bottom.

Before commencing a description of the several modes adopted for attaining either of the above ends, it should be borne in mind that, according as constitutions vary in their degrees of stability, so in the same ratio will the effects of inflammation vary in those same constitutions; I propose, therefore, to consider the modes of treatment *seriatim*, taking the milder measures first, and I would lay down as a general rule of treatment that it is not advisable to resort to severe measures to attain a result when milder ones will answer the same purpose.

In children the application of the ordinary evaporating lotions frequently succeeds in effecting a cure, probably due materially to the good feeding or tonic treatment with rest resorted to at the same time; spontaneous disappearance is not uncommon in children, and has been spoken of in connection

with adults. I have never been so fortunate as to have seen an example, but Bryant, in his "Practice of Surgery," cites one case which he believes to have been one; but some of the cases quoted of spontaneous disappearance are ones actually cured by inflammation which has been set up by some accidental violence, or by inflammation extending from the urethra to the testicle, and thence to the tunica vaginalis. One case of spontaneous disappearance cured by inflammation resulting from external violence is quoted by Sir A. Cooper at page 257 in his work on "The Structure and Diseases of the Testis." The external application of iodine, blistering fluid, and mercurial inunction, have occasionally proved successful remedies in recent cases, but so rarely, and with, in addition, so much discomfort and pain, that it is but seldom resorted to.

Acupuncture is eminently successful, especially in young children; the hydrocele is punctured in two or three places with a broad needle, the flat of the needle being turned at right angles to the puncture before being withdrawn, allowing the hydrocelic fluid to percolate into the cellular tissue external to the tunica vaginalis, substituting "an anasarca of the scrotum for a dropsy of the tunica vaginalis," the fluid being subsequently removed by absorption.

The cure results from the same cause as that by tapping, the withdrawal of the fluid allowing the tunica vaginalis to recover its lost balance between secretion and absorption. To say that the cure of the disease in these cases is effected by the support and compression afforded to the vaginal sac by the infiltration of the surrounding cellular tissue is, I believe, a mistake, as I have never yet known a hydrocele cured by compression.

Acupuncture is a mode of treatment which is simulated in nature by the accidental rupture of the tunica vaginalis and escape of the hydrocelic fluid into the surrounding cellular tissue, for when a hydrocele is of large size jumping from a height or a blow or bruise will readily burst it. This is followed by œdema of the penis and scrotum, and affords a striking analogy to what takes place when in the injection of iodine some of the injection is inadvertently thrown into the surrounding cellular tissue external to the tunica vaginalis.

In tapping, the following things have to be avoided—wounding the testicle, puncturing any superficial vein, injuring the spermatic vessels situated posteriorly, and separating the parietal layer of the tunica vaginalis from the scrotal tissues by slow puncture.

Tapping may be performed with supplemental means, such as irritating the internal surface of the tunica vaginalis by scratching it with the end of the canula, or by passing up the canula a probe coated with nitrate of silver and applying this

to the surface of the tunica vaginalis, the latter being the better operation of the two and frequently productive of the best results.

A somewhat similar plan has been tried by Italian surgeons in the application after tapping of the negative pole of a battery to the inner surface of the tunica vaginalis, the positive pole being applied to the external surface of the scrotum.

Injections may also be used in connection with tapping, of what description is a matter of taste, any irritating fluid which is of sufficient strength to set up some inflammation of the tunica vaginalis answering the purpose. Cold water, milk, port wine, spirits of wine, a solution of alum or sulphate of zinc (3 grs. to ℥j), or tincture of iodine, have all been used with different results, port wine in the hands of some surgeons having succeeded where iodine has failed. The last-named remedy is the most popular since the excellent results obtained by Sir R. Martin in Calcutta. However, to place this treatment under the head of radical, and to leave out milder remedies, is to say that milder means are never radically curable, and that this mode of treatment never fails, both of which statements are incorrect.

I find on examining the records of old cases of hydrocele treated by injection of iodine that out of fifty-four cases nineteen had been previous iodine failures. Of these fifty-four treated by tapping and injection I have made inquiry, but from twenty-five only have I been able to get replies, and I find that of the twenty-five only seven have had no recurrence, whereas eighteen have recurred. It is further noticeable that of these eighteen two had failed once before.

Of iodine injected some prefer a solution, others ʒij of the pure tincture; whichever is used does not apparently much matter, the chief point being the manipulation of the sac so that the injection may be brought thoroughly in contact with the whole of the internal lining of the tunica vaginalis.

The amount of pain consequent upon the injection cannot be taken as a criterion as to the ultimate result, for facts show that not infrequently the best results are obtained in cases of the least suffering.

The operation should only be performed at the patient's own house or where no exertion has to be taken subsequently, as occasionally severe inflammation has been set up in consequence.

The subsequent treatment is also one of great importance, the patient being confined to his bed for three or four days, and the testicle supported and ice applied if pain or inflammation be excessive.

Another point of importance should be remembered, and that

is that a platinum canula be used, as the ordinary silver canula is destroyed by the action of the iodine.

If port wine be chosen, about six to eight ounces pure are injected and manipulation used as in other cases, and after having been allowed to remain in from ten to fifteen minutes withdrawn. In injecting such a large quantity there may enter not infrequently a certain amount of air which gives the characteristic crackling sensation of emphysema, but this result is, however, of no great importance, and the air is soon absorbed.

The injection of a mixture of equal parts of carbolic acid and glycerine has been tried by Dr. Lewis, of Philadelphia, who considers it more certain in its action and less painful than iodine.

Under the head of secondary consequences, besides emphysema previously mentioned, there are refilling of the sac, orchitis, and gangrene.—*St. Thomas's Hospital Reports, vol.* vi., *p.* 111.

54.—ON A REMARKABLE CASE OF SPERMATOCELE OCCURRING IN MR. LISTER'S PRACTICE IN THE ROYAL INFIRMARY, EDINBURGH.

By JAMES BAKER, Esq., M.B., L.R.C.S., late Resident Surgeon, Clinical Surgical Wards, Royal Infirmary, Edinburgh.

The following case of spermatocele presents so many points of interest as to warrant a short account of it being published.

On the 5th May, 1876, a man about 60 years of age, named H. W., was admitted into the surgical wards of the Edinburgh Royal Infirmary, under the care of Professor Lister. The reason that he had gone to Edinburgh to seek surgical advice was that he had a swelling of the scrotum, which, he stated, he had first noticed about four years previously; since that time it had gradually increased in size. So far as he could remember, it had not been caused either by injury or overstraining.

On making a careful examination, there was found to be a large swelling on the left side of the scrotum, extending into the abdomen, along the inguinal canal. It was somewhat conical in shape, the apex being downwards. On percussion, the line of absolute dulness extended laterally from one inch to the right of the spine of the pubes to a point over Poupart's ligament, four inches to the left of the pubic spine, and vertically upwards for two and a quarter inches, measuring from midway between the anterior superior spine and the symphysis. The swelling was tense and firm. It did not disappear on lying down, nor increase in size on coughing, though it presented a marked impulse. On placing the fingers of one hand anywhere within the area of absolute dulness mentioned above, and pressing

with the fingers of the other hand on the scrotal swelling, a sense of fluctuation was obtained with great distinctness. The testicle was to the front, and about one inch above the lower margin of the scrotum.

On May 8th, Mr. Lister, using a small canula and trocar, tapped the swelling posteriorly, and evacuated seventeen ounces of a turbid, almost milky-looking fluid. Two drachms of the tincture of iodine of the Edinburgh Pharmacopœia were then injected, and thoroughly diffused throughout the whole cavity. On the following day the patient expressed himself as feeling perfectly well, and without any pain. The scrotum was considerably larger than immediately after the operation, but there was no redness of the skin. On the 10th, although he had no uneasiness when the parts were kept at rest, yet on pressure being made over the inguinal canal, pain was felt. That part of the abdomen in which, before the fluid had been drawn off, a sense of fluctuation could be obtained, was found on percussion to be resonant. The scrotum, which at this time appeared to be as prominent as ever, was noticed four days afterwards to be distinctly decreasing in size. It became less and less, until, on the 20th of May, according to the patient's account, it was smaller than he ever remembered it to have been. Fluctuation could still be elicited, but not higher than the level of the pubic bone in the inguinal canal. Three days afterwards, rather more than a fortnight after the operation, the patient was discharged from the hospital, perfectly well, never having presented any symptoms of constitutional disturbance.

It should be stated that, immediately after the fluid was evacuated, a portion of it was examined microscopically, and was found to be crowded with spermatozoa; the rest was placed on one side in a glass vessel. Twenty-four hours afterwards there was a sediment about a tenth of an inch in thickness at the bottom of the vessel, composed of spermatozoa, which, when seen under the microscope, exhibited vital movements.

It will at once be seen that the case was one of considerable interest.

Not only was the quantity of fluid unusually great, but the distance the sac extended above Poupart's ligament was especially noteworthy, being so largely out of proportion to the amount of fluid in the scrotum.

Another very interesting feature of the case was the fact that spermatozoa were observed *alive* in the fluid, and that so late as *twenty-four hours after its evacuation*. I have only succeeded in finding on record two cases of spermatocele, in which spermatozoa were seen alive in the fluid drawn off. One is a case of Mr. Liston's, alluded to by Mr. Curling in his work "On

the diseases of the Testis;" the other is reported by M. Paul Dauvé in the Gazette des Hôpitaux (21st March 1867), and mentioned in the Edinburgh Medical Journal (Nov. 1867). In the former of these two cases, the time after its evacuation at which the fluid was examined is not stated; in the latter, moving spermatozoa were noticed seven hours after the operation.

One other point in the case, the details of which I have related, deserves mention; and that is, the admirable result of using the strong tincture of iodine of the Edinburgh Pharmacopœia. From the almost unvarying success attending its use by Mr. Syme and Mr. Lister, it may be looked upon, if not as a certain cure, at any rate as greatly superior to the weaker preparations of the same drug.—*Edinburgh Medical Journal, June* 1877, p. 1085.

DISEASES OF THE SKIN.

55.—ON THE TREATMENT OF PSORIASIS.

By Dr. BALMANNO SQUIRE, Surgeon to the British Hospital for Diseases of the Skin, &c.

Phosphorus has recently awakened fresh attention as a therapeutical agent. It has especially been recommended by Dr. Broadbent as a remedy for leucocythæmia. This suggestion, however, after a particularly patient investigation of it, appears to have fallen through. Dr. Broadbent incidentally remarked, in a discussion which ensued at the Clinical Society, on some improvement which seemed to him to have taken place in a psoriasis which occurred as a complication of one of his cases. It is possible that the discouragement which attended the further investigation of the action of phosphorus in leucocythæmia may have been the cause of this incidental suggestion having been neglected. However, phosphorus had, I believe, prior to that time been tested by Dr. Hardy, of Paris, with results which afforded him some encouragement; and, subsequently to the date of the case I am about to report, it has been tried by Dr. Whipham at St. George's Hospital, but with what result it is not quite easy to understand from his description. However, since his paper (published in the Medical Times of September 22nd, 1877) is mainly devoted to the confirmation of my original advocacy of chrysophanic acid ointment in psoriasis, on this account possibly the effect of the phosphorus has been apparently less carefully attended to by him.

During the month of March, Dr. Whipham gave three times a day to a girl of 15, who had psoriasis, a pill containing one-twentieth of a grain of phosphorus. On April 1st, he "found that the psoriasis was rapidly disappearing. The improvement,

however, was of very short duration, and, on May 31st, 1877, the eruption was extending on the limbs and trunk;" but Dr. Whipham leaves one in uncertain doubt as to when the phosphorus was left off. Further on in his paper, he again refers to this same case thus: "The psoriasis was disappearing under the use of phosphorus, which was commenced in March, 1877. By the end of May, however, the disease was nearly as bad as ever, and it was evident that the drug was of no use in relieving her ailment." One is, therefore, left with this choice: either that the good effect of the phosphorus went off because the use of the phosphorus itself was discontinued, or that the phosphorus, like Penelope, undid in May what it had done in April, or at least would not do in May what it had done in April. Dr. Whipham eventually cured his patient of what on May 31st was "a copious eruption of psoriasis over the trunk, arms, and legs," by the exclusive use of chrysophanic acid ointment, with the following result; namely, that, "on June 21st, exactly three weeks after the commencement of chrysophanic acid ointment," he found that, "with the exception of one or two spots, each rather less than the size of a pea, on each wrist, she was quite free from all trace of the skin-disease." On July 22nd, he again "saw her, and found all traces of the eruption gone and her skin natural. She had discontinued the ointment for some weeks." He adds that "it was not without a feeling of despair that I had recourse to chrysophanic acid; the result, however, and the rapidity with which that result was brought about surprised me extremely, a surprise which is not lessened by the fact that the girl had suffered from the skin-disease for five years and a quarter at the time when the acid was first employed, and that she was entirely free from psoriasis in twenty-one days." Dr. Whipham's surprise was only natural. The efficacy of chrysophanic acid in psoriasis is certainly one of the most astonishing facts in modern therapeutics. I refer incidentally to this part of his paper because it was in this Journal that chrysophanic acid was first made known to the medical world as a remedy of the utmost efficacy in psoriasis, and, for another reason: because it fell to my good fortune to make that particular discovery. I regard Dr. Whipham's observations as an important addition to the other confirmatory evidence which, prior to his paper, had already appeared in the columns of this Journal. The wide publicity which was given to my observations by their appearance in the Journal has caused the remedy to be now in extensive use for the treatment of psoriasis in all parts of the world, whilst one drawback to its use which I had feared, has now been removed. I refer to its price. Chrysophanic acid, which, in December last, could only be obtained at the price of ten shillings an ounce, is now sold

for four shillings an ounce. I have little doubt but that it will speedily become much cheaper.

It will be seen from the following report that, in February and March of this year, I had a case very similar to that of Dr. Whipham, which latter extended from March to July, and that the treatment was very similar in the two cases.

Betsy D., aged 13½, was sent under my care as an in-patient of the British Hospital for Diseases of the Skin by her medical attendant, Mr. Essex of Pontypool, in Wales. She had been affected with psoriasis for only two months; but the skin of all her limbs and body was copiously covered with psoriasis, the patches on the posterior aspect of the arms and forearms being the largest of all, and many of them being of very considerable size. She had also two or three very inconsiderable spots of psoriasis on her face.

On February 22, after a careful map had been taken of every part of the eruption by means of a complete set of my "outline drawings," she began to take phosphorus "perles;" that is to say, the little French capsules of that name, which contain each one-thirtieth of a grain of phosphorus dissolved in oil, and which are to be readily obtained of any chemist. She commenced by taking only one of them three times a day.

Feb. 23. The dose was increased to two perles three times a day.

Feb. 26. The girl had taken the increased dose for a few times; but complained of severe and long continued pain at the epigastrium after each administration. The dose was now reduced to one perle three times a day.

March 6. On this, the twelfth day of treatment, many of the smaller patches had almost completely disappeared, and the others, even the larger ones, had lost their scales to a great extent; had become fainter in colour, and flattened at their central portions, leaving only raised margins. Since February 26th, she had taken only one perle for a dose. No pain had been felt. She was now ordered two perles for a dose again.

March 8. She had now taken six doses, each of two perles, without bad effect, until this morning, when, on taking a walk after her dose, she complained of pain in the stomach. The dose was, therefore, reduced again to one perle.

March 14. She had taken one perle since the last report till now. The eruption was certainly much fainter, but scarcely any additional patches had completely disappeared.

March 27. She had taken *two* perles three times a day until now since March 14. Now the patches on the chest and upper part of the back had nearly all entirely disappeared, *i.e.*, they could not any longer be identified by means of the map taken on February 22. The largest patches of all, viz.,

those on the forearms, had quite vanished, except at the actual margins, leaving only a slightly livid blue stain, and being quite free from desquamation. Many of the patches on the thighs were gone for the greater part of their area. Those on the legs had undergone the least alteration, but have lost their scales. The diseased area, which used to itch very much, had not done so for the past two or three weeks, except quite recently on the knees only, where a few small fresh patches had appeared. The perles have caused no pain in the stomach and no diarrhœa. She was now ordered to take three perles three times a day. She had not washed since the commencement of the treatment. This regulation was enforced in order that any removal of scales might be clearly due to the action of the phosphorus alone.

April 3. All of the large patches on the arms had now lost their margins, which were broken and simply dotted, and the general condition of the eruption seemed improved, though a few fresh spots had appeared, while others had gone. As regarded the buttocks, and the outer surfaces of the thighs also, and the legs (the latter more especially), the eruption was somewhat more copious than before. The perles caused no inconvenience. She was now ordered to have four perles, instead of three, three times a day.

April 12. She had taken four perles for seven days only, when pain in the stomach came on. From that time, the perles were altogether discontinued. The eruption did not seem to have varied notably. The impression produced on my mind was, that the phosphorus had attained its maximum of effect, or nearly so; or, anyhow, that it was a much slower remedy than efficient local applications often proved to be. She was now ordered to discontinue the phosphorus, and use only chrysophanic acid ointment (acidi chrysophanici ʒ ij; adipis ℥ j).

April 21. Very considerable improvement was obvious, only faint traces of the eruption remaining, except on the nates and on the legs below the knees.

May 9. Every portion of the eruption had disappeared, the only traces remaining of it being faint stains on the front of the legs and on the knees and elbows.

May 22. Since May 9, she had used the chrysophanic acid ointment (after first washing the skin each time with soft soap and warm water) twice a day to the legs only. No inflammation had resulted from this application, and the patient was everywhere quite free from all traces of the eruption.

Commentary.—It will be seen from the report that this case, not only as regards the nature and extent of it, but also as to the treatment pursued and the result of that treatment, very closely

resembles Dr. Whipham's case, and that it occurred at about the same time. Each patient was a healthy girl at about the age of puberty. In both cases, the eruption was very copious, although in Dr. Whipham's case it was of much longer standing than in mine. But that circumstance, according to my experience, makes little or no difference as to the difficulty of curing the disease, although I am aware that the contrary opinion is generally entertained. In both instances, the case was treated at first by phosphorus alone.

Dr. Whipham's case was treated by means of one-seventh of a grain of phosphorus in the twenty-four hours throughout (for apparently two months), with marked improvement for the first month, but with a return to the original condition at the end of the second month. By the way, is Dr. Whipham quite sure that his patient continued to take the pills? I am sure that my patient took the perles. Mine was an in-patient, and the matron of the hospital administered in person every single dose. Dr. Whipham's patient was an out-patient, and phosphorus pills are apt to cause disagreeable eructations tasting of phosphorus.

My case was treated by one-tenth of a grain of phosphorus in the twenty-four hours for the first twenty days, during four of which the dose had been increased to one-fifth of a grain *per diem.* During the next fourteen days, the dose was maintained at a fifth of a grain in the day. For the ensuing eleven days, the dose was augmented to three-tenths, *i.e.*, nearly a third of a grain a day; and, for the remaining seven days, the dose was increased to two-fifths, or nearly half a grain a day; making in all fifty-two days of treatment by phosphorus; namely, about the same period as Dr. Whipham's course of phosphorus treatment; my patient, however, taking, on the whole, considerably more phosphorus than did Dr. Whipham's. The result of the phosphorus in my case was that, after thirty-three days' use of it, the patient had during the entire period steadily improved, so that, at the end of that time, she had lost the greater portion, or at the least quite one-half of the original area of her eruption as it had existed at the commencement of the treatment.

During the next fourteen days of phosphorus treatment, notwithstanding an increase of the dose, the eruption for the first seven days even increased somewhat, and, for the remaining seven days, remained at about a standstill.

The conclusion I draw from the two cases is that, after about a month's employment of the remedy, the antagonism of phosphorus to psoriasis finds its equilibrium; and that the antagonism in question, although real and obvious, has, nevertheless, a limit which falls short of the complete cure of the disease. Nevertheless, I regard phosphorus as an important

and valuable addition to our means of curing psoriasis, and I am induced to think, from the results of further experiments that I have since made with it, that it may be found to be an internal remedy of greater efficacy than arsenic in the treatment of this disease. However, as I said before of chrysophanic acid, the value of it is a question to be determined, not by the results obtained by one or two observers, but by the general verdict of the profession.

I ought here to draw attention to the fact that my case shows that the dose of phosphorus, when even, as here, it is at first tolerated only with difficulty, may be *gradually* increased even in the case of a child to a dose considerably beyond the limit which is commonly assigned to it. In short, that, if caution be exercised, four times the ordinary (one-thirtieth of a grain) dose, namely, as much as one-eighth of a grain three times a day, may be quite safely given without inconvenience of any kind. I have since given this latter dose in a large number of cases of psoriasis.

As to the chrysophanic acid ointment treatment with which both Dr. Whipham and myself made amends for the deficiencies of phosphorus, Dr. Whipham's patient was nearly cured by it in three weeks, and mine in nine days. In both cases, after a further use of the ointment (Dr. Whipham seeing his patient a month and I eighteen days subsequently), we found our patients quite free from eruption.—*British Medical Journal, Nov.* 3, 1877, *p.* 620.

56.—ON CYSTS.

By Francis Mason, Esq., F.R.C.S., Surgeon and Lecturer at St. Thomas's Hospital, London.

It will be beside our purpose to enter minutely into the development of cystic growths; but it may be convenient, in order that we may thoroughly understand their condition, to divide them into two classes, and speak first of those that result from an accumulation of the contents in a pre-existing or normal cyst, and secondly of those that commence afresh as new or abnormal formations.

In the first class, there are the so-called *retention-cysts;* and of these we have one typical example in the case of a woman aged about 40, who had between twenty and thirty *sebaceous cysts* on the scalp, which we removed between us. These cysts arise from the occlusion of the hair-follicles, and, as was the case in this woman, are very frequently hereditary. I know a lady who is a sort of martyr to such excrescences, having always one or two on hand ready for operation. Several of her family are similarly predisposed; thus her mother, two

sisters, and a brother, and her son, are alike subject to such cystic tumours. Their removal is very simple. The best plan, I think, is to transfix the cyst with a sharp knife, and to turn it out with a scoop, which should be carefully insinuated at the angle of the incision, and so used as to go gently round the circumference of the cyst, the operator bearing in mind the size and shape of the tumour. No force should be employed; for, simple as such operations appear, they are not unfrequently followed by a good deal of inflammatory action, and occasionally by erysipelas. So much stress do I put on the importance of not using roughness, that I recommend, in those cases in which adhesions exist, that a few light touches of the knife should be used, keeping quite close to the cyst, rather than the growth should be dragged away with violence. Immediately the cyst is enucleated, a pad of dry lint should be applied; and, as a rule, the wound heals by primary union.

The diagnosis of these cysts, when they occur on the trunk or extremities, is not usually difficult. Sometimes, however, a small lump of fat, if situated on the face, especially the forehead, will closely simulate a cystic tumour; but the fatty growth is usually flatter and more doughy to the touch, whereas the cystic tumour is more dome-shaped, and there is a sensation of fluctuation communicated to the finger. In cystic tumours, moreover, a little black spot may often be observed, which is the orifice of the obstructed duct. On squeezing the tumour, a little of the contents may be made to exude, as in the case of a girl whom we saw the other day amongst the out-patients. In this instance, the cyst was situated on the back between the scapulæ. It is no unfrequent occurrence to find that patients who have such cysts on an exposed part, such as the face, and who are indisposed to undergo the slight ordeal of an operation, can keep the growth within the limit of becoming decency by gently expressing the contents from time to time; but the cyst is never cured unless it be removed by the surgeon, or be obliterated by inflammatory action.

The second kind of retention-cyst, or *mucous* cyst, is exemplified in a man who is now under notice, and who had a cyst about the size of a filbert, which had been growing for eight months and was situated on the left side of the lower lip. Whether it was caused by the irritation of decayed teeth I know not; but it projected, you will remember, through a space caused by the absence of the lateral incisor and canine teeth in the upper jaw. Such cysts may be excised; but in this case I made a free incision into it, evacuating a slightly viscid fluid; and I touched the lining surface with caustic. The patient is now nearly well.

Speaking of this case reminds me of several others that we have treated, and which may be classed third amongst the retention-cysts. I allude to cases of *ranula*. If once carefully observed, a ranula will never be forgotten. Situated immediately under the anterior part of the tongue, it will be recognised as a tense, semi-translucent, bluish swelling, fluctuating, and having in some cases well marked veins ramifying over its surface. It is generally of the size of a filbert or a walnut, but may attain an enormous size; thus M. Boinet relates a case in which the tumour filled the mouth, and a large portion projected from that cavity. The four incisors, two canine, and first molars of the lower jaw had been displaced by the pressure; and the patient, besides being almost starved to death, was more than once threatened with suffocation. In this case, the cyst was extirpated, and the patient recovered.

There is some difference of opinion as to the manner in which a ranula has its origin, and I will not occupy your time further than to say that some pathologists believe that it is simply an obstruction of the ducts of the mucous glands, precisely resembling the mucous cysts just described. Others think it is a dilatation of the sublingual or submaxillary duct; or, again, that, the duct having burst, the contents escape into the surrounding textures, and are there confined in a new cyst formed of condensed connective tissue. Other observers attribute it to an enlargement of the small bursæ that exist between the muscles of the tongue.

The treatment of these cysts consists of making a free incision into them, when the characteristic gelatinous mass exudes. To effect a radical cure, it is necessary to remove a piece of the cyst-wall, so as to prevent a reaccumulation of the contents; and, in using the scissors for this purpose, care should be taken to attack that part of the cyst which is least vascular. It is easy to open a vein of some size, and I have witnessed on more than one occasion considerable hemorrhage by oozing after this apparently very simple proceeding.

Included in this class of retention-cysts is that in which there is an *obstruction of the ducts of the mammary glands*. We have had three such cases lately, one of some size, from which a good half-pint of creamy material was evacuated by incision. In this list, too, we may include examples of *encysted hydrocele*. It will suffice to refer to one only of the three cases that have been recently under our observation. The man was twenty-eight years of age, and had had a swelling of the size of a currant at the top of his testicle ever since he could remember. About twelve months before his admission, he received some injury to the part, and from that time the swelling gradually increased to its present size, which is about the

size of a walnut. This case was interesting in a diagnostic point of view. There was no impulse on coughing; and the cord could be distinctly felt above the tumour, showing that the case was not one of hernia. The testicle could be distinctly felt below and anteriorly, indicating that it was not an ordinary hydrocele of the tunica vaginalis. But there was a distinct circumscribed swelling, apparently connected with the epididymis; and we formed our diagnosis from the position of the swelling, and anticipated the character of the fluid to be evacuated. The fluid had, as we expected, a slightly turbid appearance, looking as if a few drops of milk had been mixed with water. There was scarcely a trace of albumen in it; and, when examined with the microscope, we found myriads of spermatozoa. The treatment adopted in this case was to make a free incision and introduce lint, so as to promote granulations.

The second class of cysts is that known as the *exudation-cysts*, in which an excessive accumulation takes place in a pre-existing cyst which is unprovided with an excretory duct. The different diseases of normal bursæ in the body may be taken as a good type of this form of exudation-cyst, and we are fortunate in being enabled to watch four cases which illustrate their nature very well. They are all affections of the bursa patellæ. In the first—a girl aged 18, a housemaid—there is a large painless swelling situated over the right patella. She has been kneeling a good deal of late, and there is considerable effusion in the bursa. This is improving by rest and the application of iodine paint. The second case is one in which the bursa is in a state of suppuration. The treatment here was to make a free incision, not in the middle line, but at the side of the swelling, so as to obviate the pocketing of matter which often occurs in such cases. The diagnosis between a suppurating bursa patellæ and an inflamed knee-joint should be particularly noted; but the two diseases may be distinguished by remembering that, in the case of inflamed bursa, the skin is acutely painful; the patella itself is masked; and that the little dip or depression on each side of the knee is more or less retained. In suppuration of the joint, on the other hand, the patella is easily recognised by the touch, and perhaps floats. The depression on each side above referred to is lost; but the chief point is the excessive constitutional disturbance that co-exists. The third case is one of chronic enlargemeut of the bursa patellæ, which I removed by operation. So far as the tumour itself is concerned, there is nothing remarkable save the extreme thickness of the walls of the cyst; but this is by no means unusual in such cases. As regards the operation, however, you may have noted that I took care to have the limb placed on a splint before the operation, and operated with the splint applied. This may

appear a small matter; but I am persuaded it is a wise precaution, as the knee-joint is kept quiet during and immediately after the operation.

Other examples of these exudation-cysts are found in cases of hydrocele, or in the so-called ganglions which appear in connection with tendons around the wrist-joints; but, as you are familiar with their nature, I need not now refer to them.

The third variety of cyst is the *extravasation-cyst*, and you had an opportunity of seeing an example of this not long ago. It was one of hæmatocele. You will remember the man who was recently under my care with a hydrocele, and I tapped him twice. He came again in about three months with this story, that the fluid in the hydrocele had, as before, gradually accumulated; but that, the day prior to his application, as he was wheeling his barrow—for he was a costermonger—the shaft struck him on the scrotum. When we saw him there was a tense swelling of the part, which had a bruised mottled appearance. As the tension was increasing, I made a free incision and turned out the clot, and secured the bleeding vessel. The cyst healed by granulation, and a radical cure was effected, not only of the hæmatocele, but of the hydrocele also.

And now I have to say a few words as to those cysts which have, so to speak, an independent growth or origin. You will have understood that in the cases to which I have referred there has been a pre-existing cyst; but in those of which I shall now speak there is an entirely new formation, which surrounds the growth, whether it be of a solid or cystic character. The man in Albert Ward, who had a fatty tumour on the arm, was a good example of this newly-formed cyst, for the growth was included in a capsule, as such growths usually are; and this capsule was formed by condensed connective tissue. The remarks I made as to using no roughness in removing sebaceous tumours of the scalp apply with, I think, equal if not greater force in removing fatty tumours. It is usual to make one single incision, and then to tear the growth away. Here, again, I warn you as to the impropriety of rough handling, which is almost certain to be followed by considerable inflammatory action. It is better here, as in the case of sebaceous tumours of the scalp, to apply the knife lightly to any strong adhesions, rather than run the risk of bruising the parts by wrenching the growth away. Then a new cyst may form from the mere irritation of the part, such cyst or bursa being an entirely new growth. It may constitute a capsule around a foreign body, as often happens in cases of gunshot injury, in which a bullet will lie innocently imbedded in the subcutaneous tissue. I know a gentleman who can point to twelve or more shot so placed under the skin of the face. But the character of the foreign

body is a matter of indifference; it may be a piece of wood, a pebble, or a thorn. Some few months ago, we had under observation a collection of extravasated blood as large as a fœtal head, situated on the buttock. In dealing with such a case, it is well to try for a time to effect absorption by the application of evaporating lotions; or, if further treatment be required, to empty the cyst by a series of tappings, and, if necessary, to lay it open, so as to allow it to granulate from the bottom. We adopted the last method of practice in this case, and the patient made a good recovery.—*British Medical Journal, Oct.* 13, 1877, *p.* 511.

57.—A LARGE CARBUNCLE ON THE BACK OF THE HEAD AND NECK TREATED BY MULTIPLE INCISIONS.

By Montgomery A. Ward, Esq., M.B., M.Ch., Univ. Dublin, Assistant Surgeon to the Adelaide Hospital, Dublin.

Surgeons of the present day are by no means unanimous regarding the best local treatment for carbuncles: some pursue one particular method to the exclusion of all other plans of treatment, while others adopt different methods in different cases. As the subject of their treatment, more particularly of those situated on the back of the head, is one of absorbing interest, especially since so eminent an authority as Sir Astley Cooper has stated that "carbuncle does well, except when situated on the head or neck. Though persons recover from carbuncles of an enormous size upon the back, yet very small ones on the head or neck will often destroy—indeed I never saw a patient who recovered from any considerable carbuncle upon the head; in these cases there is effusion upon the brain, producing compression."

I thought that the particulars of a case which occurred in my private practice, and which was successfully treated by multiple incisions, might possibly prove interesting, especially as it was the largest carbuncle situated on the back of the head that I have ever seen or heard of, in which a complete recovery ensued.

Before describing the treatment which I adopted in this particular case, I wish briefly to refer, for the sake of comparison, to the other plans most generally pursued at the present time, and endeavour to show that no invariable rule can possibly be laid down in the treatment of this affection.

1. The plan termed compression, so ably advocated by the late Mr. O'Ferrall, and more recently by the late Mr. M. H. Collis, is, I think, admirably suited for carbuncles of a small size. I have frequently used it with marked success in such cases in patients attending the extern department of the

Adelaide Hospital; but I believe it to be impracticable in any large carbuncle situated on the back of the head.

2. The crucial incision is still a favourite remedy with a great many surgeons. In many cases it answers admirably, but I think it would be hazardous to adopt this plan in a large carbuncle extending from one parotid region to another, if a cut should be made from sound skin to sound skin—a rule laid down by its advocates. I have used this method with success in carbuncles of a moderate size, situated on the back. Two of the great objections to its use—viz., the danger of hemorrhage and the subsequent formation of a large suppurating sore—do not hold good in the plan that I recommend.

3. The method of treatment by caustics is strongly advocated by many—*e.g.*, Mr. Bryant, in his "Surgery," states that he adopts no other plan. I have never used it, neither have I ever seen it used, but I have lately seen a case in which this mode was adopted, and the appearance which the back of the neck presents is anything but satisfactory, the whole of the integument having been destroyed. The neck presents the appearance of a cicatrix so common after burns. Now, I maintain that it is most important to save the integument as far as possible; when caustic is used much must be destroyed, which can be saved in the plan which I propose.

4. With regard to the plan of treatment by simple poulticing, I have had no personal experience, but I am aware that it has proved most successful in the practice of my colleague, Dr. Richardson.

5. M. Gosselin recommends the plan of making punctures into the carbuncle, and then evacuating them by means of compression. This procedure I have never seen adopted.

The other modes of treatment I shall pass over, as they have at present few, if any, advocates.

The plan which I propose is to make one incision about an inch and a-half or two inches long, as the case may be, over the centre of the carbuncle when the slough has formed, and from four to six counter-openings round the central one, varying in size from a quarter to half an inch; then plug the openings with strips of lint steeped in some stimulating application, and lay a poultice over all, dressing the carbuncle in the same way every day until all the sloughs have separated.

The advantages which I claim for this method are—

1st, and most important, all the integument is preserved.

2nd. It has all the advantages of the mode of treatment by crucial incision, without any of the disadvantages of that method.

3rd. There is almost entire freedom from hemorrhage.

4th. The avoidance of a large, open, suppurating sore.

5th. Just sufficient space for removing the dead cellular tissue.

6th. Very slight cicatricial marking.

I shall now briefly narrate the particulars of a case in which I employed this treatment:—

Case I.—Mr. ——, a strong, well-made, active man, who is between fifty and sixty years of age, and who has always led a very regular and temperate life, has been under my care for more than a year, suffering from successive crops of boils on the back, buttocks, and backs of the thighs; some of these were large, and I was obliged to open them with a knife. As soon as he thought he was well of one crop, another would make their appearance. During the whole of this period his state of health was anything but satisfactory, his bowels were very obstinate, and his urine constantly full of lithates; besides, his mind was extremely agitated by business transactions; on more than one occasion I strongly recommended him a complete change of air and scene, advising him to take a trip to Lisdoonvarna for the purpose of drinking the waters in order to remove if possible the gouty diathesis from which he was evidently suffering, but he could not be persuaded to leave town.

On March 8th, 1877, I was asked to see him again for what he thought was another boil on the back of the neck. On making an examination, I at once ascertained that he was getting a carbuncle on the nape of the neck, and extending for a short distance upon the occipital bone. On making inquiries I found that it had been coming on for a week or ten days, and that he had been using poultices. I at once shaved the hair all round from the back of his head, and adopted the treatment by compression. This treatment I regularly carried out until the 13th inst., but still the disease daily made headway, and was rapidly extending in all directions. On this evening Dr. Edward Hamilton, ex-President of the Royal College of Surgeons of Ireland, and Dr. Frederick Alcock Nixon, saw him in consultation with me. Dr. Hamilton recommended the treatment by compression to be continued. This treatment was continued for three days longer, but still the carbuncle advanced, and was rapidly assuming a gangrenous aspect, especially over the right parotid region; it now extended from the spinous process of the fifth cervical vertebra to two inches above the occipital protuberance—in fact, it very nearly reached the crown of the head and from one mastoid process and parotid region to another. He was now in a most alarming condition, and his brain was evidently becoming affected. At last, on March the 16th, Mr. Edward Ledwich, an old friend of his, saw him in consultation with Dr. Hamilton, Dr. Nixon, and myself, when we unanimously determined to cut, as the only

chance of saving his life. As the crucial incision was out of the question, in consequence of the enormous size and situation of the carbuncle, I introduced a director into one of the openings, and made an incision over the centre of the mass where it was most boggy, for about two inches or a little more. A quantity of pus escaped from this opening, to the evident relief of the patient; at a subsequent period I made five counter-openings around and at a distance from the central cut; from all these openings pus freely came, and they were plugged with strips of lint smeared over with elemi ointment, and a poultice laid over all. After the lapse of a week from the time of cutting, the dead cellular tissue began to separate, and at every dressing for the succeeding fortnight I removed, with the forceps, large pieces of dead tissue. When all the slough was removed, on looking through the central cut there was a large cavity plainly visible, extending from at least two inches above the occipital protuberance to the spinous process of the fifth cervical vertebra, and from one mastoid process and parotid region to the other, exposing the superficial layer of muscles. This cavity was daily syringed with a carbolic lotion, and plugged with lint covered with elemi ointment. It is unnecessary to give minutely the subsequent treatment of this patient, which was conducted on general principles; suffice it to say that this large cavity gradually filled up; that all the integument has been preserved, with the exception of a small piece about the size of a farthing, which formed a nipple-shaped projection at the inferior extremity of the central cut, and which, strange to say, skinned over on its internal surface; consequently, as it was a source of irritation, I removed it with the knife. At the time I now write (August 1st) my patient is quite well, the central cut having perfectly healed. I ceased all dressing on yesterday. He is very slightly marked, and proves an exception to the rule laid down by Sir Astley Cooper.

I should mention that for some days before, and for at least ten days after he was cut, I was very much afraid of either blood-poisoning or compression of the brain, for during that period he was in a very drowsy, semi-unconscious state, though when roused up he would speak in a perfectly collected manner; he has quite forgotten everything that happened during that time.

With regard to the constitutional treatment, I kept up his strength in every possible way with plenty of the best beef-tea, milk, &c.; at one time stimulants were freely given, and the medicines chiefly relied upon were 5-grain doses of quinine, alternated with the bark and ammonia effervescing mixture.—*Dublin Journal of Medical Science, Sept.* 1877, *p.* 207.

58.—NOTES OF A CLINICAL LECTURE ON ACNE.

By JONATHAN HUTCHINSON, Esq., F.R.C.S., Senior Surgeon to the London Hospital; Surgeon to the Moorfields Ophthalmic Hospital.

When the face is covered with pimples, some of which are red, some contain pus, and others show only black points in their centres—all kinds being present, and all slow in progress,—it is commonly agreed to call the condition Acne. If the spots are angry and suppurate quickly, it is acne pustulosa; if they are small, very florid, and not prone to suppurate, it is acne rosacea; if there is great thickening about them, and again little tendency to suppurate, it is acne tuberculata; if there are numerous black points to be seen, it is acne punctata; lastly, if no one of these features be in excess of the others, it is common acne—acne vulgaris. Now, let us first understand clearly that these various adjectives do not denote different diseases, but merely different conditions of the same disease, which may be frequently met with in one and the same case. Next, we will observe that all forms of acne are inflammations of sebaceous follicles. I have already said that, when a follicle inflames, three results ensue—a thickening of its gland tissue, deposit and congestion of the cellular tissue around it, and accumulation of its secretion in its interior. Now, we have in acne all shades of variety as to these three results. Everyone is familiar with the little black dots so frequent in the skin of the face of those who have rather coarse complexions. In degree they may perhaps be found in the skins of most persons, especially about the nose. If you squeeze them, little black-headed "maggots" are ejected. These maggots, or grubs, are not living, but consist of half-dried sebaceous matter, which had accumulated in the cavity of the gland, and which has been moulded into the pellet form in passing through the constricted opening. The black head is the end of the pellet, which, having been long exposed at the mouth of the duct, has gathered soot.

It is not always that the end of the pellet gets blackened; sometimes, and especially in young persons, the mouth of the follicle is closed by a delicate membrane, and then the secretion collected beneath it is seen under its transparent covering, and remains quite white. In infants this distension of closed follicles constitutes what used to be known as strophulus albidus; in adults it is more frequently seen on the eyelids than on other parts.

Sometimes the interior of the follicle suppurates, and, after removal of the pellet, pus escapes. This constitutes pustular acne.

It is a peculiar feature of the condition known as acne, that at one and the same time, in the same patient, you will find the follicles in all stages of disease, some simply distended and free from material irritation, others congested also, others suppurating. In this it differs much from lichen.

Acne is emphatically a disease of coarse skins; or rather, perhaps, we ought to say that the term "coarse skin" usually applies to integument in which the sebaceous follicles are larger than ordinary, and have gaping mouths. This causes the skin to look rough and pitted. It is a state of skin the tendency to which is often hereditary, and it is thus often seen in several members of the same family.

Acne spots cause more annoyance on the face than elsewhere, and hence an exaggerated impression as to their great relative frequency on this part. Although there is no doubt that the face and shoulders are their usual sites, yet, if you will examine the general surface of acne patients, you will very frequently find the spots, in smaller numbers, on the trunk and the upper arms also.

Having asserted that all persons of coarse skin are liable to have their sebaceous follicles take on occasionally the acne inflammation, we may suitably ask what are the causes which induce the more severe forms of the disease. For clinical purposes we may recognise acne chiefly in two forms—first, the acne of young persons; and second, the acne of those past middle life. It is in young persons that we meet chiefly with the pustular, punctate, and vulgaris types, whilst in the elderly we encounter the acne rosacea and tuberculata. Respecting the acne of the young, there is a very widespread opinion that it is usually the result of sexual disturbance. I have no doubt that this belief is well founded to some extent, but we must beware of exaggerating it. The eruption is chiefly met with in young celibates, whilst it is very rare under the age of puberty, and is often benefited by marriage. It is possible, however, that its comparative rarity in the married may after all be a coincidence and not a sequence, and that we ought to consider it not so much a disease peculiar to celibacy as to the special age at which a large majority of the population are celibates. It may certainly occur before puberty. I have seen it not very infrequently in children, and once in a very marked form in the face of an infant of six months. It is also frequent in married persons of both sexes, and sometimes originates after marriage. I have known it occur in ladies who were bearing children, and in whom the sexual functions appeared to be in perfect activity.

Making full allowance for a considerable number of acne

cases in which there appears to be no sexual cause, there are yet, I think, good grounds for accepting the general belief that in a majority of instances such is the fact. The remarkable influence which the sexual functions exercise upon the general health and upon the state of the nervous system is amongst the secrets known unto all men. That they should have the power of making the sebaceous glands of the skin enlarge and suppurate is certainly, if thought about, one of the most strange. I suspect that, when it occurs, it is brought about through the agency of the nervous system rather than of the blood. Women who are not liable to acne at other times sometimes have a few spots appear at each menstrual period, and that whilst in excellent general health. I have been assured by gentlemen liable to nocturnal emissions that they invariably had an increase of acne spots after such occurrences, and sometimes so immediately, that it was impossible to believe that any material change in the blood had occurred. In other cases sexual intercourse may produce the same result.

It is certainly not in cases of extreme sexual exhaustion that acne is most common. I have seen many such patients, both with and without spermatorrhœa, who had not a spot of acne, but, on the contrary, had skins which were perfectly smooth—in some instances florid, in others earthy pale. It is, perhaps, rather a condition of sexual irritability than of exhaustion which produces acne. I do not think that the severity of the acne eruption bears any relation to the degree of sexual disturbance. In the worst cases that I have seen the patients often seemed to be in good health.

To dismiss this subject, we may remark that the prescriber ought, in respect to the acne of celibates, to bear in mind the possibility of a sexual cause. He will advise the adoption of measures likely to improve the general vigour, he will caution against any possible causes of debility, and he may, in some instances, suggest matrimony as the remedy most likely to prove successful. Derangements of stomach and liver stand in about the same relation to the acne of middle-aged persons that sexual causes do to the acne of early life. Manifest dyspepsia (often the result of intemperance) is present in perhaps half the cases of acne rosacea, whilst in the other half it is exceedingly difficult to assign any cause. The same remark applies also to the indurated and tuberous form of acne, which produces the thickened, bossy condition of skin familiarly known as "grog-blossoms," and usually considered to be proof of intemperance. In many cases such suspicion is most unjust. At any rate, of this you may be sure—that in persons congenitally of coarse skin very slight indulgence at the table may produce results in the way of acne, which would not ensue in

others whatever the amount of provocation afforded. As I asserted in reference to sexual exhaustion, so I may say here again, it is common enough to see the most intemperate escape scot-free. Nothing would be more unjust than to allow ourselves to entertain the belief in the one form of acne, that it is certainly due to sexual causes, or in the other that intemperance or gluttony is the cause. We will freely admit a frequent connexion, but strongly deny that it is invariable. For the worst forms of acne of either variety you will be able to discover in the patient's state of health or antecedents no cause whatever, and you will be compelled, in considering your measures of treatment, to regard it chiefly as a local disease.

The rules for the constitutional treatment of acne patients follow easily from what we have said. If the patient be young he should be made to use a cold bath every morning, to take plenty of exercise, to live liberally as regards meat diet, with a fair allowance of stimulants; and he should be cautioned or encouraged, as the case may be, in reference to sexual matters. As to medicines, a long course of small doses of arsenic will often be of great use, If constipation be present, the habitual use of a chalybeate aperient should be prescribed. You may do all this, however, most sedulously, and gain nothing whatever, if you neglect local measures; whilst with the latter only, and without any change in the patient's habits, you may often get an acne eruption so nearly well that he will regard it gratefully as a cure. The chief local measure consists in destroying, by means of a fluid caustic, the inflamed follicles. With a fine-pointed glass brush, or a bit of soft wood cut to a point, you touch the inflamed spots from day to day. Take great care not to apply too much. In the left hand should be a roll of blotting-paper with which to absorb the fluid if it has been deposited too abundantly. The best fluid to use is the acid nitrate of mercury. It will usually be necessary to repeat the touching once a week for a month or two, carefully seeking out every fresh spot. After that the patient should still see you once a month, in order that the cure may be kept up. The acid thus used does not leave larger scars than the spots would themselves do.

In acne rosacea the use of the caustic will again serve an excellent purpose. You may not only touch the spots themselves, but also pencil out the stray vessels which add so much to the patient's disfigurement. He, or more usually she, will gladly exchange a few slight and scarcely perceptible scars for the angry and very suspicious-looking redness of face which the disease causes.—*Medical Times and Gazette, July* 28, 1877, *p.* 88.

59.—CASE OF ACNE ROSACEA TREATED BY OINTMENT OF CHRYSOPHANIC ACID.

By Dr. BALMANNO SQUIRE, Surgeon to the British Hospital for Diseases of the Skin.

A lady, aged forty-five, residing in one of the Midland counties, had been affected with acne rosacea for about a year and a half, when she came up to London to be treated for it. She is approaching the menopause—that is to say, for the past two or three years her periods have been irregular. However, her general health is apparently perfect, and she declares that she has always enjoyed the best of health. She is a brunette of sturdy build and hearty appearance. Her face is her only misfortune. This region presents not merely the blotchy patches of discoloration which are characteristic of some varieties of acne rosacea, nor that copious sprinkling of minute pimples which represents another common phase of the disease, but rather what may be termed the tuberculous variety of acne rosacea—that is to say, the papules, or rather tubercules, are individually large; not that their sebaceous core forms any considerable portion of their bulk (as is wont to be the case in the indurated phase of "acne juvenilis"), but that the elevated induration which encloses the small core is notably developed. These tubercules (several of which are the size of split peas), although they are mostly smaller, occupy very abundantly the forehead, the cheeks, and chin, and also that portion of the skin of the neck which lies immediately under the lower border or "*base*" of the lower jaw.

She was treated with chrysophanic acid ointment as an external application to the face, and with glycerole of nitrate of bismuth as an internal remedy. No other remedy, external or internal, was used from first to last. She commenced treatment on January 19, 1877. On February 27 she presented herself quite free from any trace of her former eruption. I attribute the alteration she experienced purely to the action of the chrysophanic acid ointment. There was no indication whatever for the exhibition of bismuth; the patient's digestion was in no way out of order; but I was engaged at the time in making further observations on the effect of my glycerole of the nitrate of bismuth, the preparation and physical properties of which have already been fully described in this journal.

In the case of this patient, a dose of the glycerole containing four grains of the nitrate of bismuth, given three times a day for a few weeks, produced no appreciable effect of any kind.

As to the ointment, it consisted at the first of twenty grains of chrysophanic acid dissolved in an ounce of lard at the

temperature of an oil-bath. For the last ten days of the treatment, however, the strength of the ointment was raised to that of forty grains of chrysophanic acid to the ounce of lard. The ointment in either case was regularly, three times a day, rubbed well in all over the face, avoiding only the eyelids and the lips. From the beginning to the end the patient never experienced any smarting from this energetic treatment. However, occasionally the face became a little puffy, as if slightly swollen. Throughout this treatment the face became more or less stained by the action of the ointment, but it was not *very* much stained. The complexion of a field labourer about autumn time is often quite as dark as this patient's face was at any time of the treatment. The stain proved, of course, quite transient, passing away completely after a few days' discontinuance of the ointment.

Commentary.—The case above related bears on some points in dispute as to the action of two new remedies. Of the glycerole of nitrate of bismuth it was generally prophesied that it would prove a very sharp and acrid medicine: but in this case a fair dose of it given for a long while did not appear to be at all a disagreeable remedy. Of the chrysophanic acid ointment it has been said by some that it is dangerous to use it to the face, and by others that even when used to the tougher regions of the skin its strength ought not to exceed a scruple of the acid to the ounce of lard, and even then its use ought to be cautiously limited to one or two, or at the most but very few, applications. Now, in this case an ointment of forty grains to the ounce, well made by one of the best chemists in this city, was energetically rubbed in over the whole of the face three times a day for thirty times in all, without producing any sensation of smarting, nor causing more swelling than a very moderate puffiness of the face. Then the staining of the skin has been spoken of as a great disadvantage. "Patients," it is said, "object to this very much." Now, this patient did not make any difficulty of that kind. The case illustrates, moreover, quite a new field for the employment of chrysophanic acid. I have already pointed out that it is a serviceable remedy in cases of psoriasis. To this I have now to add that it is capable on occasion of curing acne rosacea."—*Medical Times and Gazette, June* 23, 1877, *p.* 665.

60.—THE TREATMENT OF SMALL-POX BY SALICYLIC ACID.

By ENGLEDUE PRIDEAUX, Esq., late Resident Medical Officer, Derby Small-pox Hospital.

The great object in small-pox is to modify the disease as much as possible, and to arrest it in its second and third stages,

so as to prevent that destruction of tissue and subsequent cicatrisation which are so liable to follow, and thus deform all the beauty of the human features, often making them almost unrecognisable. The more intense the disease, the greater the eruption (except where it kills on its first onset), and the more universal it is, and, as an accompaniment, the higher the temperature. Now, the exceeding high temperature denotes excessive action on the part of certain of the tissues, an excessive tissue-change, which we may regard as inflammatory in character. Regarding the disease as of germ origin, or caused by a morbific ferment, we have, first, an introduction of the germs, and then their propagation with exceeding rapidity, accompanied by, according to Beale, a rapid proliferation of bioplasmic particles constituting the febrile state. As the disease progresses, the poison is eliminated from the tissues by the skin and mucous membranes, producing the eruption and forming foci of inflammation local in character, which may be termed a dermatitis, and which may go on to the formation of pus and destruction of tissue, always found in the severer forms of the disease.

Regarding these germs, or minute bioplasmic particles, as low forms of life, if we are able to introduce into the blood and tissues something that is fatal to their activity, we shall necessarily mitigate a disease depending in great measure upon that activity; it is necessary, then, that we should use a drug that will be absorbed into the blood without undergoing material change, and of which we may use considerable quantities without ill effect. It appeared to me that in salicylic acid we have a drug that fulfils all the required indications; it is a powerful antiseptic, and is very fatal to all the lowest forms of life; it passes into the blood materially unchanged, probably in the form of the neutral salts of soda and potash, and appears in the urine in the form of salicyluric acid, and may be detected by the addition of a few drops of tinct. ferri perchloridi, which produces a characteristic violet colour. When pure it may be given in large doses, as much as half an ounce in twenty-four hours; when given in these large doses it is apt to produce a marked depression, but this I find is obviated by giving small doses of carbonate of ammonia. The best mode of administering it I find to be in solution with carbonate of ammonia and bicarbonate of soda in the proportion of five grains of each of these to twenty grains of the acid. This mixture is most pleasant to the taste and perfectly unirritating to the intestinal canal, and in some sixty or seventy cases I have never known it produce sickness.

At the Small-pox Hospital, Derby, I have lately treated twenty-nine cases of small-pox with salicylic acid, all of whom

recovered, and all with the most marked results, both as to the progress of the disease and the subsequent pitting. This latter, I may say, has not occurred, the disease under this treatment being arrested in the third stage, so that little or no suppuration is produced; the temperature is brought rapidly down and kept down, so that no secondary fever occurs. Of these cases, four were unvaccinated; eight cases were confluent, and one hemorrhagic or malignant. The notes of this last case I subjoin:—

A. E. T., æt. 14, was brought into the hospital on the evening of the day of the eruption, in a state of extreme prostration. She had vomited blood in large quantities twice; had a temperature of 106°·1, and was violently delirious. She was at once placed in a tepid bath of 90°, allowed to remain in ten minutes, and ordered salicylic acid, ten grains every four hours. The next morning her temperature was 101°; the bath was repeated during the day; her temperature rose to 103°; the salicylic acid was increased to fifteen grains, and one drachm of spirit. ammon. aromat. added to each dose, whilst a draught of chloral was administered at night. The next morning the temperature was 101°, and it gradually fell, until on the following day, the third day after admission and the fourth day of the rash, it was 99°, and it never rose afterwards. The pustules were confluent, not only all over the face, but over the whole of the body; they were black from hemorrhage in a great many places, whilst there were hemorrhagic petechiæ scattered about in various parts. However she did remarkably well, the contents of the pustules were in great measure reabsorbed, and the skin simply thrown off. She was discharged cured in a month, although retained in the hospital as nurse for the children.

All the other cases did equally well; and I have since been informed of cases in private practice where equally good results have taken place under the same treatment. I found a mixture of glycerine, gelatine, carbolic acid, and water very useful in painting the surface of the body; it quite prevents the odour of the disease, and entirely alleviates the troublesome itching, so much so that patients ask to have it by the bed-side so that they may paint themselves frequently. After a few days, and during the convalescent stage, I find that the citrate of iron and ammonia is a very useful adjunct to the salicylic-acid mixture, with which it is quite compatible, and forms an agreeable mixture. Whilst desquamation is going on, a daily hot bath, containing some two pounds of washing soda, aids the process very much.

Unfortunately, as a rule, patients are not brought into hospital until the second stage of the disease, when the rash has appeared, and consequently one loses the opportunity of

checking its extent, which can only be effected by recognising and treating the disease at the commencement of its first stage; but that we can by this treatment modify the disease in the end of the second and during the third stage, and prevent in great measure the formation of pus, I am quite certain, and thus we may almost entirely prevent the disastrous after-effects of the complaint.—*Medical Examiner, May* 31, 1877, *p.* 429.

61.—COLLODION FLEXILE IN CASES OF ECZEMA.

By Dr. Henry Lawson, Assistant-Physician to, and Lecturer on Physiology at St. Mary's Hospital.

There are few diseases more troublesome to the patient, and more unsatisfactory to the physician, than that of eczema. The sufferer goes on from month to month, now getting a little better, then becoming worse; while he not unfrequently throws undue blame on the shoulders of his medical adviser, and occasionally he goes from one physician to another in the vain hope of getting better advice. If, therefore, we can find any means of rapidly bringing about a cure, we shall be entitled to the thanks, at all events, of the sufferers. And there is good ground to hope that we have found out a means not only of affording considerable relief, but of actually removing the disease in two of its troublesome forms. Of course I do not imagine that the attempt is new, for doubtless it has been often made before, though it is not mentioned in some of our best handbooks on skin diseases. But there is reason to fear that it has not been carefully tried, and that hence the attempts made with it have proved failures. In my hands, however, in two bad cases of eczema—E. genitale and E. capitis—collodion has shown itself so valuable a remedial agent that I lose no time in publishing the result, in order that others may try it, and see what the consequences are likely to be. I shall now describe the two cases.

The first case was one of E. genitale. The patient, M. E., was a woman aged about forty-seven years, married, and the mother of several children. She was a florid woman, of an active temperament, well nourished, of moderate habits of life, tolerably cleanly, and with a pulse strong and full and about 74 in the minute. She had lost her courses about two years ago; and, indeed, her general appearance was not such as led me to commiserate her very much. However, an examination of the patient showed that she had been suffering a good deal. The whole of the neighbourhood of the perineum, of the parts about the vulva, and of the inner margin of both thighs, were covered with an eruption. And what was its nature? It is difficult to describe it. It had a reddish or reddish-purple

aspect, which was of course caused by the injection of the parts with blood; and it could be seen that certain parts were slightly raised; while over the whole surface was a sort of semi-transparent glutinous liquid mass, with here and there some scaly particles of epidermis. It did not smell badly, though the entire amount of surface exposed must have been quite a square foot; but it was accompanied by great pain, heat, and secretion of liquid matter. Indeed, the patient declared that it made her life a perfect misery.

Well, I first tried tar water, and with some success, but not enough, for after a fortnight she was nearly as bad as on the first day I saw her, and she had been fourteen months suffering under this disease. So I resolved to try the collodion flexile. I placed her on the sofa, and proceeded to literally cover the diseased parts with collodion, and then I put a second layer over the first. I next directed her to put on this material twice or oftener if needful every day, and to come to me in a week and report progress. At the same time I forbade her to take tea, coffee, or malt liquors, but to substitute cocoa or milk, and to take a little whisky if she desired it. Finally, I ordered her a compound colocynth pill, with podophyllin, to be taken occasionally at night.

When, at the end of a week, this patient came to me I was absolutely astounded at the progress she had made. There was not at all the same amount of secretion over the surface, and it seemed paler, while it had not extended in the least degree. She said she felt she was getting better, and it was not nearly so painful as it had been. Of course, I simply repeated the prescription, and when she came again in a fortnight all appearance of liquid on the surface had disappeared. The extent of the affected parts had diminished, so had the pain, which was now nearly *nil*. In fact the remedy had acted most satisfactorily, and there was nothing to do but repeat it. This course was followed out by the patient for about two months, at the end of which she presented herself completely cured of the painful E. genitale.

Now this case, I believe, was cured solely by the covering which the collodion supplied, in that way completely preventing the action of the air on the Malpighian layers of the epidermis, and thus allowing the upper layers to be formed beneath its protective influence. I think, also, that it was because I put on so good a layer of collodion at first, and because I cautioned the patient not to spare it, but to put it on abundantly, that it operated so successfully. At all events, it wrought a complete cure in a relatively short space of time.

E. capitis.—The patient who presented this disease was a boy aged ten years (H. S.) He had none of the signs of scrofula

which are usual in this form of disease. He seemed plump and well-fed. His teeth were bad, but he did not complain of toothache. He had beautiful hair, which hung down in handsome ringlets. But it was matted together over the left parietal bone, which was the locality of the eruption. This could not be well seen at first, and I confess that on my first survey of the case, which was made very hurriedly, I put it down as one of scabies, and ordered sulphur ointment, at the same time telling the mother that she must have the hair cut from all the diseased parts, and cut closely, at once. She did so, and brought the boy to me in a few days, when, to my surprise, the ointment had done no manner of good; indeed his head appeared, if anything rather worse than better. However, now I could examine it, and I did so with the aid of that admirable pocket lens of Browning's,* and I found not a trace of acari, which fact, as well as my examination of the head, which presented a raw and inflamed surface, partly scabby and partly covered with a transparent ichor, led me to conclude that it was a case of eczema capitis.

The line of treatment to be adopted was indicated by my former experience; at least, I regarded it as probable that a similar mode would be equally successful. I therefore cut the hair off as closely as possible from the diseased parts and half an inch round them, and applied collodion flexile freely, putting on a second layer after the first had dried, and making the mother watch carefully the mode of operation. I then told her to apply it twice daily exactly as I had done it, taking great care to leave not a particle of the diseased surface exposed to the air; and having given him a little rhubarb and soda powder, to be taken in the mornings, and ordered him to have cocoa instead of tea or coffee, and abundance of beef-tea, I told him to come again in a week.

He did not come to me for ten days, and when he did, I confess I was somewhat disappointed at the result. The head was not worse, but it certainly had not been improved. I suspect my instructions were not rigidly carried out, and any-one who knows anything of the mass of the poorer patients in London will readily admit that this is not only possible, but is very highly probable. However, I told his mother that if he was not better next time I should dismiss him and let him try someone else; and at the same time I told him to come to me in a week, and to continue the treatment the same as I before directed. He came next week, and I was glad to see that there were marked signs of improvement. The surface presented a much smaller amount of raw surface, and the portions which

* This is a new form of pocket lens made by Browning, of the Strand. It is in many respects superior to either the Stanhope or Coddrington glass.

had retained the collodion upon them appeared—where the collodion was not thick, for in some cases it was impossible to see through it—much less red than they had been.

It would, of course, be idle to report his various succeeding visits, as they all showed each an improvement on the one before. It will be sufficient to state that in about seven weeks he was so far recovered that I allowed his hair to remain uncut, and ordered the collodion to be only occasionally used. I have since ascertained that he has made a complete recovery. —*Lancet, June* 23, 1877, *p.* 901.

62.—ON THE TREATMENT OF TINEA TONSURANS.

By Dr. Robert J. Lee, Senior Assistant-Physician to the Hospital for Sick Children, Great Ormond Street.

There are numerous agents which seem to have more or less active influence in the treatment of ringworm; some being advocated by some practitioners as superior to others, while these again have their own supporters. The spores of the trichophyton appear to resemble the microspores lately examined by Professor Tyndall in their obstinate resistance to destruction; and the successful treatment of cases of tinea tonsurans clearly depends on determining whether it is possible to destroy these spores, or whether, by preventing their germination for a certain period, the disease practically cures itself. The observation of some troublesome cases of ringworm which had been under various kinds of treatment without much benefit, suggested a plan of treatment which excluded the possibility of destroying the trichophyton spores, and only had for its object the arrest of proliferation of the germs. The question of the destruction of microspores is one which perhaps does not present itself as quite a different question from the prevention of their development. An example will illustrate what is meant. If we take a solution containing bacteria, such, for instance, as that in which bladders are prepared for museum purposes, the odour of which is singularly powerful, and add to it a certain quantity of carbolic acid solution of the strength of 1 in 40, we shall find that the active living organisms which exist in the former will be instantly destroyed and the odour removed. If we do the same thing with a solution of salicylic acid of full strength (water absorbs only about 1 in 400) the organisms are not destroyed and the odour is not removed; that is to say, salicylic acid will not destroy well-developed bacteria. But salicylic acid will prevent them from developing, as is proved by the fact that we may preserve animal or vegetable matter from decomposition by treating it with solution of the acid. We thus see the importance of distinguishing between agents which destroy

bacteria and microspores, and those which simply prevent their development; and there is no doubt that those who have been studying this most interesting subject by clinical, microscopical, or physical methods are well aware of the importance of ascertaining the conditions which favour or arrest the development of different species of germs; clearly a stage in the inquiry beyond that of the extent to which the germs may be destroyed by various agents.

As it is well known that some of the remedies used for ringworm are less liable to produce inflammation of the skin than others, it is most desirable to give a preference to the former, the production of inflammatory changes seeming rather to retard than promote the action of a remedy. On this principle I have, during the last twelve months, used carbolic acid, the most certain agent for the prevention of the development as well as for the destruction of microspores, with decidedly better results than were observed when iodine, tincture of the sesquichloride of iron, or any other agents had been employed, including Goa powder, which has lately been recommended as superior to most others. There is one important point which must be attended to under any circumstances; and this is, the necessity of much more frequent application of any remedy than is usually considered requisite, for the reason that most species of microspores require only a few hours to advance from one stage of development to another, and that, in order to prevent any increase in the number of the spores, though we may not be able to destroy them, it is absolutely necessary to apply the remedy at intervals of not less than six hours. The best preparation for this purpose is a combination of sulphur and olive oil in equal parts, to which carbolic acid in the proportion of two grains to the drachm is added. To prevent the contact of the fingers of the person who applies it, and who is liable, without caution, to take hold of a child by the neck or shoulders, and thus produce the disease on other parts, a small sponge or brush should be used. This must be done every four or six hours, the head being washed with Castile soap and warm water night and morning before the application of the carbolised oil. If a stronger solution of the acid be used, as, for instance, in the proportion of 1 to 10, it will be found that a certain amount of inflammation is produced, and the frequent application of such a mixture cannot long be pursued. After making various experiments of this kind, I have found the preparation given above most satisfactory, and believe that the treatment of ringworm with carbolised sulphur oil may be recommended as superior to any other in common use.

As a matter of experiment, there is no doubt as to the fact that no agent with which we are acquainted is to be compared to carbolic acid for the destruction of organic life without

destruction of organic matter, and that no agent is so useful in treating parasitic diseases of the skin, from the fact that, in proportion to its destructive action on the organisms which produce them, it is the least injurious to the cutaneous tissue.

Attention to details is of such importance in the treatment of tinea tonsurans, that it is necessary to add to the above directions the remark that the hair should be cut close with scissors, and that the oil should be rubbed into the skin for a few minutes. The treatment should be continued for at least a fortnight after the disease has apparently been cured. Either of the following prescriptions may be used. The first has the advantage of not becoming thick or dry from evaporation, while the second is cleaner and cheaper.

℞. Sulphuris precipitati, zinci oxidi, āā ʒ j; olei olivæ f. ℥ j; acidi carbolici gr. xvi.

℞. Sulphuris precipitati, zinci oxidi, āā ʒ ij; glycerini, aquæ, āā f. ʒ iij; acidi carbolici gr. xvi.—*British Medical Journal, July* 21, 1877, *p.* 74.

63.—THE HYPODERMIC TREATMENT OF BRONCHOCELE BY ERGOTINE.

By Dr. J. G. Sinclair Coghill, Physician to the Royal National Hospital for Consumption and Diseases of the Chest, Ventnor.

The therapeutic uses of secale cornutum are being rapidly extended from the comparatively humble and limited *rôle* to which, in obstetric practice, this drug was originally confined. Its marked influence on the contractile elements of the tissues of the vascular system has led to its being placed in the first rank of hæmostatic remedies. Again, indirectly, from its controlling power over the supply of materials through the circulation, its effect on the processes of nutrition furnishes indications for a much wider range of employment.

Mrs. J., a lady in her forty-ninth year, came to Ventnor in June of last year to consult me about an enlargement of the thyroid gland, which occasioned her great and increasing physical distress. She had at various times been treated by the different preparations of iodine, both internally and locally, with no effect, and had quite recently gone to Edinburgh, where the removal of the tumour by a surgical operation was urged as the only means of relief. She was a native of a district in the north of England where goitre is notoriously endemic, and, indeed, several members of her own and her husband's family have been similarly affected, but to a less degree fortunately. The enlargement of the gland, it seems, had been noticed as soon as menstruation was established, and

she thought she had remarked a distinct increase of size in the tumour at each period, but also particularly during each of her three pregnancies. The menstrual flow has always been very profuse, and at intervals within three weeks.

When I first saw her the amount of exophthalmos was very striking. The bronchocele filled up the whole space between the chin and clavicles, being especially prominent below and on the right side. The tumour felt exceedingly hard and tense, and its weight caused additional distress to the patient. An exploration with the needle showed that it was not cystic, but fibro-vascular in character. The effect of the pressure of this mass on the trachea and œsophagus threatened to be most serious, and kept the patient in a state of great alarm. It was impossible for her to swallow any solid food except in the smallest morsels, and washed down with fluids. The slightest exertion brought on violent attacks of dyspnœa.

As the usual remedies had been tried in vain, and the patient had positively declined a surgical operation, I determined to endeavour to reduce the tumour by injecting a solution of ergotine hypodermically, This treatment of tumours was, I believe, first suggested by Prof. Hildebrandt of Königsberg, and used successfully in a fibro-myoma of the uterus. He employed an injection of a watery solution of three grains of ergot in glycerine. I used in the present case the ergotine discs of Messrs. Savory and Moore, which are most convenient, and thoroughly reliable. In all sixteen injections were made over the tumour, and as closely in contact with its substance as possible. I commenced with one disc equal to one-third of a grain, and increased the amount gradually to three discs, equal to one grain of ergotine. They were simply dissolved in distilled water, and injected at blood heat. The first four injections were made daily, the next four at intervals of two days, and the others at longer intervals, the whole extending over two months.

The results were soon apparent, and were most satisfactory. The tension and dense consistence of the tumour first of all diminished rapidly, with great relief to the dyspnœa and dysphagia. The whole mass became gradually reduced in size, until the left lobe, which had always been the least, returned to its natural size, while the right and middle lobes certainly returned to half at least of their former dimensions. It is to be noted that although the injections were at first and for the most part made over the right or larger side of the tumour, the left and central portions became much more rapidly and to a comparatively greater extent reduced in size. The nutrition of the entire body seems also to have been influenced to some extent by the remedy, for although the patient was by

no means obese, she lost fourteen pounds in weight during the two months, but with no loss of strength or other deterioration of health. It is singular also that the menses did not appear for six weeks during the hypodermic treatment, thus missing one regular period, an irregularity never previously experienced by the patient, and when they did return, they were much less in quantity than usual, although large doses of the fluid extract of ergot had been previously given internally without producing any effect whatever on the discharge.

Although the patient was of an extremely nervous and irritable temperament, the general effects of the drug were at first very slight, but after a time there was observed, immediately after each injection, considerable vascular excitement, with flushing, pain and throbbing in the head. I have since quite recently heard that there has been no return of the enlargement, and that the patient is otherwise keeping quite well.

We have undoubtedly in ergot and its active principle ergotine a means of most powerfully affecting the vascular system and its contents. It is to be observed that, as a hæmostatic, its influence seems to be much more potent when introduced directly into the blood through the tissues hypodermically, than when it enters indirectly into the system by the stomach. It is but reasonable that such should be the case, if the object be to bring the remedy and the tissues to be affected into the most complete and rapid contact. Certainly in hæmoptysis it is much more efficacious when used hypodermically than when administered by the stomach. It is more than probable that its chemical instability is such that it undergoes some change of composition in the process of digestion, which may entirely alter its character, or at least seriously deteriorate its therapeutic properties. It is only in this manner we can explain the marked difference in results between the two modes of admininistration in question.—*Lancet, Aug.* 4, 1877, *p.* 158.

64.—TREATMENT OF CHRONIC ULCERS OF THE LEG.

By Dr. W. Alexander, Surgeon to the Liverpool Workhouse Hospital.

In a communication to the Medical Examiner, Dr. Alexander gives the results of some experimental researches he has had the opportunity of making on the relative value of the application of different ointments and lotions to chronic ulcers of the leg. The only unctuous application in which he has any confidence is the boracic acid ointment made after Mr. Lister's formula, of one part of boracic acid, one part white wax, two parts paraffin, and two parts almond oil, modified and softened by a little glycerine. This is most useful, he finds, in slowly healing

ulcers, keeping them clean and healthy looking for any length of time. Chloral lotion has two distinct properties, cleansing and healing, the former being its pre-eminent one. He regards it as the most generally useful lotion. In a healthy sore, where there is no mechanical obstruction to cicatrisation, the lotion most rapid in producing the desired effect is chloride of zinc. The sulphate appears to be much inferior. Much caution is required in the use of both. The lotio borosalicylica is very useful in superficial ulcers. The old spirit lotion still holds its ground as a most useful application to healthy wounds, and lotio ferri is highly advantageous in pale anæmic ulcers with watery discharge. Lead and opium lotion holds an enviable position in clean but irritable ulcers, with a deep-red and painful state of the integument around. This seems to be its special niche, as it nearly always leads to disappointment when used in other varieties of ulcer. Water-dressing, he thinks, may do well in private practice, but is dangerous where many ulcers are congregated, and he only tried it in two cases. Carbolic acid, used either as a lotion or mixed with oil, he looks upon as our sheet anchor in hospital practice, and especially in workhouse hospitals. It should be resorted to at once if pyæmia, erysipelas, or phagedæna threaten, and in dirty sores of all descriptions, but its efficacy in healing chronic ulcers is much inferior to many other applications. Mr. Hutchinson's antiphlogistic lotion, composed of acetate of lead ʒ iv., diluted acetic acid ʒ ii. ℈ ii., and rectified spirit ℥ viii., healed five out of eleven cases. Electrolysis and grafting he classes together as different modes of forcing cicatrisation in healthy granulating surfaces. When everything is favourable cicatrisation may be much hastened and facilitated thereby, but in chronic ulcers electrolysis resembles the application of the spur to the exhausted horse, and grafting is to the ulcer what over-feeding is to an exhausted stomach. He dwells on the great advantages to be derived from mechanical appliances in the treatment of ulcers, and mentions the several situations where mechanical forces prevent cicatrisation. In conclusion, he believes he is justified in laying down the following propositions regarding chronic ulcers of the leg:—(1) That an ulcer on a comparatively healthy leg will be healed by every mode of treatment that secures cleanliness, and that does not interfere with the healing process. (2) That in the majority of cases the causes preventing cicatrisation are mechanical, and can be best treated by removing or counteracting these obstacles, and that until this is done any benefit to be derived from topical applications will be but temporary and evanescent. (3) That large ulcers of the leg require a certain time to cicatrise, that periods of rest in the process are necessary for the maturing and contraction of each

fresh piece of a cicatrix, and that we ought to devote more attention to the signs that will indicate whether an indolent ulcer should be stimulated, or whether the apparent indolence is only a rest, as necessary for its future exertions as our daily sleep is to us. (4) That we must shun the over use of wet applications, the lotion should be applied to the ulcer alone, and the surrounding skin should be kept as dry as possible. (5) That where wet applications are resented by the skin or ulcer, oxide of zinc powder dredged over the limb is most useful; and where the ulcer is healing, but the surrounding skin unstable, the zinc lotion to the ulcer and the powder to the skin have seemed to me the most appropriate treatment.—*Practitioner, Sept.* 1877, *p.* 206.

AFFECTIONS OF THE EYE AND EAR.

65.—ON OPHTHALMIC THERAPEUTICS.

By Talfourd Jones, Esq., M.B. Lond., University Medical Scholar; Physician to the Breconshire Infirmary.

There are three very active medicines which are now classed together under the head of mydriatics. These are belladonna, stramonium, and hyoscyamus; and they are called mydriatics because, when applied to the eye or when given by the mouth, they cause mydriasis, *i.e.*, dilatation of the pupil. The active principles of these three mydriatics are almost identical in their action; but the one which is of the most use, and which is best known, is atropia or atropine. Atropia was discovered forty-six years ago, and most of its chief physiological actions have been long known. It is a matter for surprise, therefore, that a remedy which has been proved to possess a very remarkable therapeutic value, which is so clean, so handy, so certain, and so rapid in its action, is still by many practitioners never used at all.

It will not be necessary for my present purpose to enter into the general physiological action of atropine, but it is important that we should briefly consider its peculiar action on the eye. We now know that the movements of the iris are due solely to the action of its muscular fibres; and we also know that the dilatation of the pupil, caused by atropia, is not due to the local action of the alkaloid on the muscular tissue of the iris, but to its paralysing action on the nerve filaments, or nerve-endings in the iris. The ocular motor fibres of the third nerve supplying the circular, *i.e.*, the contractor or sphincter muscles of the pupil, are paralysed by a local application of atropine; and the filaments of the sympathetic ending in the radiating fibres, *i.e.*, the dilator pupillæ, are stimulated. We know that

division or injury of the cervical sympathetic produces contraction of the pupil and passive congestion of the eye; whence it is inferred that the sympathetic supplies the dilator pupillæ and the vaso-motor branches; hence atropine, by stimulating the sympathetic, causes dilatation of the pupil in a way distinct from its paralysing effects upon the filaments of the third nerve. It also causes contraction of the muscles, and so relieves congestion.

Atropine, when administered internally, acts probably on the eye in precisely the same way, though perhaps not to the same degree; that is, it acts on the nerve-filaments of the iris as if locally applied. Atropine also gives rise to increased arterial tension, and this rise in the blood-pressure is probably due to its action on the muscular tissue of the arterioles, whereby a contraction of the vessels ensues. As for myself, I have no doubt on this point; and, therefore, believe that it does lessen the blood-supply when applied to the eye. Atropine also acts as a local sedative to the muscular and nervous tissues of the eye; for it produces a local anæsthetic effect on the ocular filaments of the fifth nerve, and so we may fairly call it an anodyne.

It is now almost universally believed that atropine causes lessened intraocular pressure or tension, and this no doubt happens as a consequence of the lessened blood-supply. Not the least important action of atropine I have purposely left to the last. When freely applied, it causes paralysis of accommodation. Next to its mydriatic action, this is by far the most important one. In order to understand clearly the action of atropine on the accommodation, let me remind you of the action of the ciliary muscle. This muscle, muscle of accommodation, or tensor of the choroid, is known to effect those alterations in the curvature of the lens, by which the eye is adjusted or "accommodated" for vision at different distances. The nearer the object the more convex does the lens become, and *vice versâ*. And since we are more or less incessantly adapting or accommodating, by the action of this muscle, our eyes for objects at different distances, it is apparent that, for purposes of healthy vision, this muscle should be in a sound and healthy state; that it should not be overtaxed; also that, in many diseased conditions, it is essentially necessary that this muscle should have absolute rest. By the free use of atropine, we can insure this rest, and at the same time give rest to the iris, and so obtain that rest for the eye which we would wish to secure for a broken leg by putting it up in splints.

For therapeutical purposes, the sulphate of atropia is the most convenient form in which to use atropine. This salt is very soluble in water, requiring no addition of rectified spirit.

If the usual four-grain solution of the *Pharmacopœia* be dropped into the eye, it will in most cases produce in half an hour complete dilatation of the pupil. It is then that the power of accommodation becomes impaired, and near objects cannot be distinctly seen. In about an hour later, *i.e.*, an hour and a half from the instillation, there is more or less complete paralysis of accommodation, and no objects within twenty feet can be distinctly seen. When complete paralysis of accommodation is once produced, it often happens that normal accommodation does not return for a week or a fortnight. By using a weak solution of atropine, it is very easy to cause mydriasis without paralysing the accommodation; hence, for purposes of ophthalmic examinations, it is wise to use a minimum quantity of atropine; though, for therapeutic purposes, it is usually of extreme importance that the accommodation should be paralysed.

Dr. H. C. Wood of Philadelphia, in his able work on Therapeutics, quotes a short paper by Norris, the dentist of New York, which is well worth your perusal. Speaking of its anodyne action, he says: "In phlyctenular keratitis, by its local anæsthetic action on the branches of the trigeminus it diminishes the photophobia and blepharospasm, and seems to mitigate the intensity of the inflammation by its influence in contracting the ciliary vessels, thus diminishing the supply of nutritive materials carried to the cornea."

Let me now sum up in five sentences the wonderful effects of atropine upon the eye: it causes mydriasis; it lessens the blood-supply; it lessens intraocular pressure; it paralyses the accommodation; it causes local anæsthesia. Stramonium and hyoscyamus are almost identical in their action with belladonna; hyoscyamus is, however, believed to act more distinctly as a hypnotic; and most of us would use hyoscyamus for this purpose when we would not think of prescribing belladonna.

In the October number of the Practitioner for the year 1872, a very interesting and instructive paper was published by Mr. Henry Power on the internal administration of belladonna in strumous ophthalmia. In addition to the local instillation of atropine in the varied forms of strumous or scrofulous ophthalmia which are met with and called phlyctenular, pustular, and so on, he recommends that belladonna should be given internally; and he has found the most troublesome cases yield to this treatment. This plan of treatment, he acknowledged, was not a new one. I had long myself been acquainted with the value of the internal administration of belladonna in similar cases. Of late, however, I have come to the conclusion that, where the patient can be daily seen by the medical man, and a proper instillation of atropine insured, the internal

administration of belladonna is almost unnecessary. Where, however, we cannot insure a careful and efficient instillation of atropine, it is a most excellent plan to give belladonna internally, and also to apply it in the shape of paste to the orbit or forehead, care, of course, being taken to warn the child's parents of the special symptoms which would necessitate a discontinuance of the drug. In nearly all the cases of phlyctenular ophthalmia that come under my observation, I find that the local application of atropine and Cremer's pomade, together with a general attention to the constitutional state of the child, will readily effect a cure. In fact, the results of this plan of treatment are so good that I am always pleased when such a case is brought to me.

With your permission I will narrate very briefly a few typical cases illustrating the treatment which I wish to advocate.

Case 1. About two years ago, I went into an adjoining county to see a married lady who was said to be getting blind, and who was suffering so much from intense pain that her life was almost despaired of. The history of the case was this. A month before my visit, she was taken ill with neuralgic pains about the left forehead and temple. In a week the pain became almost limited to the eyeball. Then the eye became reddened; she was unable to sleep; and exposure to light intensified the pain. She sought medical advice: she was ordered to remain in a darkened room: astringent remedies were applied to the eye, then fomentations and poultices; but the patient got worse: her appetite failed and the mischief progressed. At the end of a month, twelve leeches were applied to the left temple, and for a few hours she was easier; but the next day she was even worse than before. It was then that I was requested to see her. She was lying on a sofa in a room from which every ray of light was carefully excluded: she was very weak, had a worn-out and haggard look, and kept the left eye tightly closed. The photophobia was so extreme that I failed to obtain a view of the cornea; I therefore injected, by means of a blunt hypodermic needle, a few drops of a four-grain solution of atropine between the lids, and applied a compressive bandage, and gave her a draught containing 30 grains of bromide and 10 grains of iodide of potassium, with 10 grains of chloral-hydrate. In half an hour, I was able to examine the eye, and found ulceration of the lower and inner quadrant of the left cornea, with some turbidity and much vascularity. The ulcer was marked with white lead streaks. The conjunctiva was hyperæmic; there was troublesome lacrymation; and the eyeball was somewhat tense. I instilled more atropine, and re-applied the compressive bandage. At the end of a second half-hour, the pupil was widely dilated, and the photophobia

marvellously lessened, so much so that I was able to hold a Weiss's benzoline lamp in front of the eye. She was ordered to take three times a day 15 grains of bromide with 10 grains of iodide of potassium, and at bedtime a similar draught together with 20 grains of chloral-hydrate; to take brandy and eggs, and plenty of food; and atropine was directed to be instilled three times a day. At my next visit, on the third day, there was a marked and most satisfactory improvement. She had slept well each night, and was absolutely free from pain. Cremer's pomade was now prescribed in addition to the former remedies. She continued daily to improve, and by the seventh day the ulcer had almost healed; and in less than a week she was as well as ever. Unluckily in this case an acetate of lead lotion had been used, and opaque specks and streaks of white lead remained imbedded in the new corneal tissue.

This is a good example of what we may expect to meet with in a case of long-continued and neglected neuralgia affecting the ophthalmic division of the fifth nerve. The fifth nerve is a nerve of sensation, and it also ministers to trophic functions; and it must be borne in mind that the nutrition of the eyeball is liable to suffer greatly when the fifth nerve is diseased. Had the neuralgia in this case been properly treated at the onset, all the subsequent mischief to the eye would have been prevented. Had proper neurotic medicines been given, such as those which were ultimately prescribed, the disease would have been stopped. Instead of this the pain and loss of sleep were not properly combated, and free local abstraction of blood was ventured upon, when, to my mind, a transfusion of additional blood would have been infinitely better.

This case shows also the evil that results from applying lead-lotions to an abraded or ulcerated corneal surface.

Case 2. As an example of specific disease not diagnosed, and consequently not properly treated, the following brief case is worth recording. A young girl was brought to me in 1865. She had been under medical treatment for some time; blisters had been applied behind her ears; various pain-producing lotions had been used. She gradually got worse; at last, became nearly blind. When brought to me, she could not distinguish any object, and she was obliged to be led into the room. There was in this case chronic iritis, with some lymph in the anterior chamber; and the corneæ were blurred. Suspecting specific disease, I enquired, and was soon satisfied that syphilis was at the bottom of it. Atropine was freely instilled; and iodide of potassium with perchloride of mercury, and belladonna, were given internally. In a month, the girl was well. She was recommended to go on with the iodide of mercury for some months.

Case 3. The next is a case of mild catarrhal ophthalmia accompanied with ulceration of the cornea. On November 9th, 1876, a baby three months old was brought to me with a bad eye. The mother said it began three weeks before with "a cold in the eye," and she did not think much of it until she saw a white speck some six or seven days ago. I noted that there was photophobia: the lid was slightly swollen, with a little sticky muco-purulent discharge. On the cornea, there was a circular ulcer about a line in diameter, containing a white opaque slough. There was haziness of the cornea for some little distance around it. Cremer's ointment was applied, and a weak atropine solution instilled; a compressive bandage was applied; and the baby was given bromide of potassium and belladonna internally. Next day, I determined, in consequence of the great difficulty met with in applying local remedies, that chloroform should be given. It acted admirably: the child left me fast asleep in its mother's arms, and remained easy the whole of the day. Whilst under the influence of chloroform, it was easy to examine the upper lid, which could not be inspected before. It was swollen, red, with some discharge. A weak solution of nitrate of silver was dropped in, and atropine again applied. Belladonna paste was applied externally. Henceforth chloroform was regularly administered morning and evening. Cremer's ointment was substituted for the nitrate of silver. By the sixth day, there was no swelling of the lid; no discharge. The pupil looked bright, and the ulcer was healing. On this day, a belladonna rash broke out on the head, neck, and back. The belladonna was withheld, and next day the rash was gone. By the twelfth day, the child was well.

I quote this case more particularly to show the value of chloroform in such cases. Chloroform or ether should be employed in all cases where, in consequence of photophobia and muscular spasm, a proper examination cannot otherwise be made. Chloroform acts readily on the pupil. At first, it causes contraction; later on, dilatation, by its paralysing effects on the motor oculi. It has appeared to me that chloroform is not only useful in enabling one to carefully and easily examine the eye and to apply local remedies in a way that would in many cases be impossible without it, but that its sedative or anodyne action continues for some time, often many hours, after the general effects pass away. Ringer states that Sir James Simpson had observed that a few drops of chloroform put on the palm of the hand and held near a photophobic eye, so that the vapour entered the eye, would enable it to bear the light without pain.

Case 4. In December 1874, a young lady came to me from the neighbourhood of Swansea, suffering from phlyctenular ophthalmia. Her eyes had been bad off and on for three years.

She was obliged to leave school, and has to wear blue glasses. There was extreme photophobia; several small ulcers were to be seen on the margin of the cornea, numerous vessels, and the conjunctival papillæ were red and prominent. There was a total absence of anything like scrofulosis. Atropine was fully applied; one-eighth of a grain of belladonna extract was ordered to be taken three times a day, and Cremer's pomade was applied night and morning. In a fortnight, the eyes were nearly well. Atropine was now used but once a day, though the belladonna was increased to half a grain. In February, she returned home, and was instructed to use weak atropine solution and Cremer's ointment, and to take quarter-grain doses of belladonna. Wishing to prevent a recurrence of the disease, I advised her to use the remedies for some months. In June, the belladonna was reduced to one-eighth of a grain. The binoxide of mercury ointment was regularly applied until the end of June. The eyes were then quite well, and the conjunctival papillæ were no longer visible. From that day to this, now two years, she has had no further trouble.

Case 5. The next case is a good example of the so-called scrofulous ophthalmia. The girl, ten years old, presented the general aspect of scrofulosis. She had been suffering from bad eyes for more than a year. The mother told me that she had consulted several doctors in this county, and in Glamorganshire; that all sorts of remedies had been tried; that so many painful things had been done to the child that she quite dreaded coming to me. On inquiry, I found that nearly all the local applications that had been used had given her pain; also that she had been freely and repeatedly leeched and blistered. The large scars behind the ears, extending down the neck, showed how severely she had been treated. In fact, she was permanently disfigured.

I noted that there were extreme photophobia, a little mucopurulent discharge, ulcers on the cornea, and granular lids. Atropine was instilled; belladonna paste was ordered to be applied around the orbit, and belladonna and iodide of potassium were given internally. The lids were touched with lapis divinus, which is a handy and useful remedy composed of equal parts of sulphate of copper, nitrate of potash, and alum, moulded into sticks. Cod-liver oil and syrup of iodide of iron were also prescribed. This was on November 15th; on the 17th, *i.e.*, in two days, there was a marked improvement, and in six weeks, under this treatment, she perfectly recovered.

Case 6. Some time ago, a young man consulted me who, for some months, had been suffering from phlyctenular ophthalmia. He was getting no better, worse in fact, under medical treatment, and he was informed by his doctor that nothing further could be done. He inquired if it would be any use

consulting an oculist; he was told that no oculist could do him any further good. A friend of mine happened to see him and sent him to me. On examination, several minute ulcers and small pustules were seen on the cornea, with a cloudy and dull pupil. There was photophobia and ciliary neuralgia. I told him that, if he would but promise to come and see me regularly, and would do exactly all that he was ordered, he would probably be well in three weeks; but that he must expect to have some corneal opacity left.

He promised, and he was most exact in performance. I applied my usual remedies, *i.e.*, atropine, Cremer's pomade, and a compressive bandage, and gave him iron. At the end of a fortnight, he was almost well, and, before the end of the three weeks, was quite free from trouble. He was then told to discontinue all the remedies except the ointment, and this he was advised to use occasionally for some months. A faint cloudy opacity of the cornea remained, as was prognosticated.

In the treatment of most of the commoner forms of disease of the conjunctiva, cornea, and iris, the most successful practitioner will be he who, in addition to making a wise use of the neurotic and constitutional remedies of which we have spoken, also knows best how to apply local remedies. Let me briefly say that, where the conjunctiva is inflamed, metallic astringents are indicated, and that the best are nitrate of silver and acetate of lead.

When the cornea is inflamed or ulcerated, there is no local remedy equal to atropine; and metallic astringents must be avoided, though, in certain stages and under certain conditions, a weak red precipitate ointment is often of service; and, when this is indicated, there is no preparation, to my mind, so convenient, so useful, and so handy as Cremer's pomade.

When the iris is inflamed, atropine must be freely instilled, and, for this disease, it is the local remedy. When both conjunctiva and cornea are inflamed, the practitioner must learn how best to apply astringents to the mucous membrane and yet avoid irritating the cornea.—*British Medical Jouraal, July* 28, 1877, *p.* 100.

66.—ON THE MYDRIATIC AND OTHER TOPICAL EFFECTS OF THE APPLICATION OF GELSEMINA TO THE HUMAN EYE.

By John Tweedy, Esq., F.R.C.S., Clinical Assistant, Royal London Ophthalmic Hospital, Moorfields.

About eighteen months ago I was asked by Dr. Ringer to complement a series of investigations, initiated by Mr. Murrell and himself on the topical action of the alkaloid of gelseminum

sempervirens upon the human eye. These gentlemen had already observed the curious fact that while the internal administration of the tincture of gelseminum in doses sufficiently large to produce toxic effects caused contraction of the pupil, the local application of a solution of the hydrochlorate of gelsemina to the conjunctiva was invariably followed by dilatation of the pupil. In one instance which I saw with Dr. Ringer the pupils were contracted to a pin's point, and there was a strong double convergent squint from the internal administration of large and frequent doses of gelseminum, and in several other cases the contraction was well marked.

It is worthy of remark that whereas the commonly recognised dilators of the pupil are obtained from some member of the natural order Solanaceæ, a powerful mydriatic has now been discovered in gelseminum, which is botanically allied to Strychnos Nux Vomica, and is, indeed, said by many authorities to belong to the same natural order—namely, Loganiaceæ.

There can, I think, be no doubt that gelsemina, locally applied, affects the terminations of the sixth nerve, so that the strength of the external rectus is impaired; but I have been unable to determine whether the internal rectus becomes really stronger or gains power only relatively by the weakness of the antagonistic external rectus. In the experiments performed by Dr. Ringer and Mr. Murrell upon rabbits and cats, in poisoning by gelsemina, whether internally administered or locally applied, the eyeballs became prominent from paralysis of the ocular muscles. This was very marked in the rabbit. Subsequent experiments upon man showed that one of the first symptoms of the toxic effect of the internal administration of gelseminum was weakness, but not actual paralysis, of the external rectus muscle. It would appear, therefore, that gelseminum has a special affinity for the sixth nerve; although, when given in large doses, it profoundly affects also the third nerve. I could not detect any prominence of the globe from the local application of gelsemina in man, nor was the action upon the extra-ocular muscles in any instance great enough to give rise to diplopia.

One of the chief merits of gelsemina as a mydriatic has yet to be described, at least as far as the ophthalmic surgeon is concerned—and it is from this standpoint I have mostly watched the experiments. The importance which is now very properly attached to correction of anomalies of refraction, and especially abnormal regular astigmatism, necessitates the frequent and almost constant use of atropine to overcome the power of the accommodation for near objects. But against atropine there has always been the serious objection that its effects last so long that great inconvenience arises to the patient from being unable to do near work for several days after the error of re-

fraction has been estimated. If the pharmacopœal solution of sulphate of atropine has been employed, at least eight to twelve days must elapse before the accommodation returns to its normal state. With gelsemina, on the other hand, sufficient accommodation returns within ten to fifteen hours to enable a person to read newspaper type at twelve inches, and within thirty hours the accommodation will have practically returned, although the pupil may remain somewhat dilated, though not quite immobile, for several days. I can also state, from personal experience, that the mistiness and confusion of vision when the eye is fully under the influence of gelsemina is nothing like so great as when atropine has been used.

Practically, it may be stated that gelsemina locally applied readily dilates the pupil, and, when used of sufficient strength, temporarily overcomes the accommodation. It is preferable to atropine in cases where the power of accommodation is not great, where it is necessary to overcome the accommodation for a short time only for the purpose of estimating the degree of ametropia, because its effects are more transient, and the confusion of vision during its action is less. To ensure paralysis of accommodation within three hours a solution of at least eight grains to the ounce must be used every fifteen minutes for the first hour, and every half hour afterwards.—*Lancet, June* 9, 1877, *p.* 832.

67.—ON THE REMOVAL OF FOREIGN BODIES FROM THE EAR.—NEW SPOUT SPECULUM.

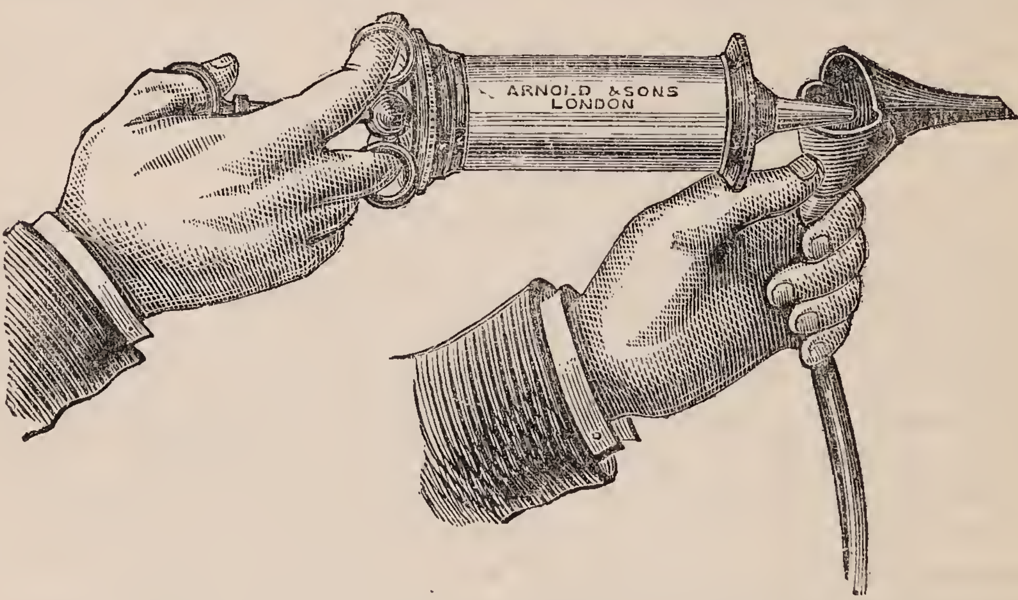
By Dr. Robert T. Cooper.

Mr. F. H. Ward, in the British Medical Journal, of Jan. 27, 1877, thus expresses his opinion upon the operation of syringing as a means of extracting foreign bodies from the ear:—"I think the main point to be observed to obtain success in the operation is to draw the pinna well upwards, in order to alter the relation of the meatus to the unyielding body which it contains, and so give a space for the entrance of the fluid. If this precaution be not observed, or *the stream be caused to impinge directly on the foreign body itself*, one can almost be induced to believe that all the syringing in the world would fail to move the mass in the slightest degree."

We can cordially acquiesce in the idea that causing a stream of water to play directly upon a foreign body, firmly impacted in the external auditory canal, will fail not merely to shift it, but to shift it *in the proper direction*, for this qualification is a very necessary one. And yet it remains an undeniable fact that, however syringing be performed, it has proved itself, if we are to judge by our own experience, and by the expressions

of approval with which it is everywhere received, a fairly successful means for removing foreign bodies from the meatus. And this arises, I am forced to believe, more from the happy natural construction of the auditory canal, as well as from the comparatively small size of the bodies that find their way into it, than from the skilful manner in which the operation of syringing is usually performed.

Taking into consideration, however, that the shape of the ordinary ear specula, as we find them in the instrument-makers —to wit, Toynbee's, Wilde's, and Gruber's—would be extremely inconvenient for such purpose, being with difficulty held should the syringe-piston be stiff or the syringe itself be too large to manage conveniently with the one hand alone, I have devised an instrument with a handle attached to it, such that while the syringe is being held with the right hand it can be steadied, while the surgeon is syringing, upon the joint of his left thumb flexed upon the handle. In this way we can play steadily upon any part of the foreign body we find to be insecure so as to get the stream of water well behind it, or, if it be necessary from the nature of the impacted mass, as sometimes happens with cerumen firmly clinging to the sides of the canal, we can concentrate our efforts upon such portion of it as seems from its fixedness to require detaching from the sides of the meatus.



As is well known it sometimes takes a long time—where, for example, the cerumen is unusually hard—to detach it, but generally speaking if we can loosen the outer portion of the mass, hardened and dry from contact with the external air, the remainder, which is usually soft, having been protected from the external air, comes away easily. Toynbee insists, and very correctly, "that the ear should be drawn backwards so as to straighten the tube; and if this cannot be effected on account

of the left hand of the surgeon being otherwise engaged, the posterior part of the meatus should be pressed backwards by the point of the syringe." But let anyone try and press backwards the wall of the meatus in the way indicated, and he will find it all but impossible to do so without influencing the direction of the stream of water coming from the syringe. This is not the case when we use pressure backwards with a speculum, as the orifice of the speculum, completely corresponding to the calibre of the auditory canal, allows of the jets of water being directed upon any one point.

Another consideration having reference to the patient's comfort, and a very necessary one, is to avoid wetting their clothes when we are syringing. Toynbee guards against this misadventure by means of his "ear-spout." "The use of an ear-spout is very serviceable during the process of syringing. It consists of a spring to pass over the head, at one end of which is a funnel to fit under the ear, down which the water can run into the basin." He then gives us a plate of his well-known and eminently useful "ear-spout" fitted on the head. The objections I find to Toynbee's ear-spout are, that if the spring is not strong enough to cause it to remain sufficiently close under the ear, some drops will now and then find their way down the patient's neck, and so defeat the object for which the spout was intended; while another, and the principal objection I urge to it, is its constituting an unnecessary addition to the requirements of the surgery—one at least that, as we shall show, may very well be dispensed with. Others use a specially constructed basin (Wilde), others an ordinary drinking tumbler half filled with the fluid to be injected, and direct the patient to press it well in under the ear; but this is objectionable as a nervous patient, instead of pressing it tightly in under the lobule of his ear, very often will jerk it from him the moment the operation of syringing begins, and to allow space for the stream of water to flow down his neck.

For these reasons the speculum, of which we give a woodcut [this drawing is faulty in giving the idea of light being obstructed by the spout-handle], is constructed with a spout-handle, an appendage that perfectly answers its two-fold requirements of a handle and a spout. We have termed this a spout-speculum, and feel convinced that any one who once uses it will not care to syringe after the old fashion, and, besides this, it has the not inconsiderable advantage, in these days of unnecessary costly instruments, of being as inexpensive as any ordinary ear speculum, and quite as useful as any of them for the purposes of an optical instrument. I may explain that my speculum is constructed in vulcanite; specula of which, as Pollitzer says (at least he is speaking of much the same

material, namely, "rubber"), have the advantage "of being much lighter, and are, therefore, borne in the meatus by the patient, without support, far more easily than the many metallic cones, and, moreover, do not occasion the unpleasant cold sensation caused by a polished metal surface. The dark ground of the inner surface favours a clear definition of the illuminated parts far more than does the slight reflection of light from the polished metal, which increases the brightness of view at the centre.

Vulcanite is a material especially adapted for the requirements of a syringing speculum, which at any moment, on a sudden movement being made by the operator, might be forcibly pressed against and, if of metal, might injure the sides of the meatus. My speculum, as will be observed from the woodcut, is bevelled off at the end, so as to spoon away the mass of foreign matter as it is making its exit from the meatus. This, of course, is not absolutely necessary, though it really is advantageous. With a bright artificial light and a laryngeal mirror on the operator's forehead, and an india-rubber tube fitting closely over the spout of the speculum, through which will run the out-going stream of water into an adjoining basin, one can syringe for any reasonable time with this instrument, with the greatest possible comfort to oneself and the least possible inconvenience to one's patient.

To turn now to another consideration regarding the removal of foreign bodies from the ear—namely, the relative advantage of the ordinary ear-scoop and the syringe for this purpose.

Mr. Walter Rivington, in the British Medical Journal, March 18th, and again in that of December 16th, 1876, expresses his belief that "the ear-scoop should be banished from the surgical *armamentarium*." We agree most cordially with this opinion; the ear-scoop is an awkward, unsatisfactory, and dangerous instrument, but how can we pretend to displace our instruments unless we come forward with efficient substitutes; and this, so far as I am in possession of the facts, has not been done by this very intelligent surgeon.

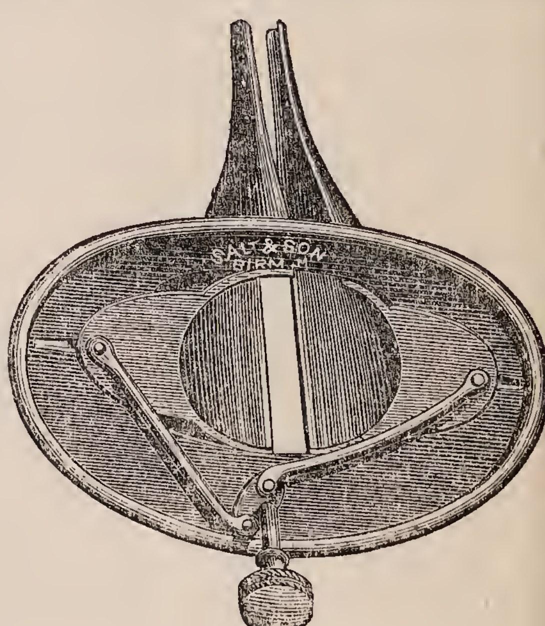
Those who have followed us thus far will perceive that the principles upon which syringing, as a means for removing foreign bodies from the meatus, are based, are that the column of water from behind will drive the foreign body outwards, and that, therefore, if the water cannot insinuate itself between the sides of the meatus and the impacted body, the syringing alone cannot possibly effect its object, and that the hitherto very general favour in which it is held is owing to the difference in shape between the inner extremity compared with the middle region of the auditory canal—so much so that in syringing, as we generally perform it, a foreign body impacted mid-

way in the canal is at the commencement of the operation driven further in, prior to the pressure of the column of water from behind it acting so as to impel it outwards. Now, if these premises be true—and any practical surgeon, not to say aurist, will know that they are so—it follows that our chief aim in syringing ought to be to get a mass of water behind the impacted body, so that by exercising pressure upon it from inside, the force of the water will drive it out along the auditory canal. To accomplish this object effectually, I have suggested the construction of a syringe-nozzle, made somewhat in the shape of an ordinary dressing-case ear-scoop, the ribbon-shaped opening of the bore of which is placed *above* and somewhat short of the semi-flattened scoop-like extremity, with the object of directing the stream *over* the scoop-end, so that it may pass at first between the latter and the wall of the meatus. In this way the small scoop-like extremity having been insinuated a little way between the foreign body and the upper (generally) wall of the meatus, on forcing in the water with the syringe, the stream passing over the scoop-end of the nozzle will be directed into the fundus of the auditory canal, without at all impinging upon the foreign body, all the while steadied, if need be, by pressure of the scoop upon it. The possible objection that may be urged to this is that insisted upon by Mr. Rivington, in opposition to the ordinary ear-scoop, in his paper of 16th December, 1876, where he says:—"It occupies a great deal of space, and it must be insinuated by the side of the foreign body, and reach its posterior part, to be of any service at all." In the first place, our scoop does not occupy nearly so much space as an ordinary ear-scoop, and it need not pass further than a very little way beyond the line of impaction; it is enough if it can get sufficiently far in to allow of the water passing to the far side of the foreign body, and to enable us to use gentle pressure from above upon the foreign body, and so prevent its sliding inwards during the act of syringing; for it is not difficult to believe that with the foreign body steadied, and the force of the stream of water directed against the upper wall of the auditory canal, our dependence being upon the force gained by the column of water welling-up from behind, the stream will, without injurious disturbance of the foreign body on the one part, or of the membrana tympani on the other, cause pressure to be made, a true *vis à tergo*, by the column of water pushing from behind as it accumulates against the impacted body; and this—the operator taking care to lift off the pressure from the body by gently directing the nozzle up from off it, when he knows the water has well accumulated—will allow of the water from behind driving the body outwards. But if the body impacted be composed of a hard, non-resisting

material, and be firmly wedged in, we cannot, it may be urged, obtain even the small space required for the practical carrying out of these principles. This can hardly be so, for if we give a thought to our anatomy we will find that "the whole cartilage" [of the ear] "may be looked upon as an elongated plate, the lower part of which is folded round in front so as to bring it *nearly* into contact with the upper part." So that by taking advantage of this cartilaginous deficiency in the upper wall of the tube, we are enabled, however impacted a body may be, to insert, in certainly the great majority of cases, our scoop-nozzle far enough to allow of the stream of water passing on to the tympanic extremity of the canal. More than this, if the foreign body be irregular in outline, and be calculated, by reason of its irregular formation, to do injury to the tympanum, if brought into contact with it, the being able to steady this body by the aid of our syringe-tube, and so to prevent the impacted mass getting further into the canal, is a very obvious advantage—such a body as a cockroach, which formed the subject of discussion between Mr. Rivington and Dr. Alford Nicholls, of Dominica, would be. Certain I am that with our scoop-nozzle and spout-speculum it would be, indeed, an extraordinary mass of cerumen, which would baffle all efforts at dislodgment "for twenty minutes or half an hour," and this "without removing any particles, or even causing the water to be clouded" (Toynbee). These instruments are to be had of the well-known surgical instrument makers, Messrs. Arnold and Sons, Smithfield.—*Dublin Journal, April, p.* 343.

68.—NEW DILATING SPECULUM AURIS.

We beg to introduce to the notice of our readers a new dilating speculum auris constructed by Messrs. Salt and Son, Birmingham, on the model of the tracheotomy canula designed by Mr. Wagstaffe, and described recently in the medical journals.



This instrument, being expanded by a series of levers acted upon by a large milled head, is handy, entirely under control as to the extent of dilatation, and so arranged as not to interfere with the vision, and, having a large external orifice, well polished, is capable of being highly illuminated, and affords a very distinct view of the meatus.—*Lancet, Feb.*, 1877.

SYPHILITIC AFFECTIONS.

69.—ON PRIMARY SYPHILITIC SORES.

By EDWARD COCK, Esq., Consulting Surgeon to Guy's Hospital.

Amongst the modifications and alterations which are familiar to, and are appreciated by, a practised eye, there are none so decided and so easily recognised as the induration, the result of solid effusion, so characteristic of a syphilitic sore. Hence the well-known distinctive division of sores into hard and soft. This induration varies as to the time of its advent. It is sometimes found surrounding the sore in its early stage, constituting what is called Hunter's chancre. Sometimes it is much later in its development, and not unfrequently is cotemporary with the healing of the sore, when it assumes the form of a raised gristly callus instead of the usual cicatrix of a common healed ulcer. Much stress is deservedly laid on this solid effusion and induration as establishing the syphilitic character of a sore, and rendering the occurrence of "secondary symptoms" all but a certainty. The amount and extent of induration has been said to indicate and foretell the extent and severity of the "secondary symptoms." I believe there is much fallacy in this opinion.

My experience would show that the extent of the induration connected with a syphilitic sore, and the amount of its density, is governed much more by its situation and the textures involved than by the virulence of the inoculated poison. The same poison imbibed may in one part of the organ produce the most extensive and unmistakable syphilitic induration, while in another region the solid effusion is so limited as hardly to distinguish the ulcer from an excoriation or a common "soft sore."

I have already mentioned the different localities of the penis which may become the subject of syphilitic sores, and I enumerated and distinguished them in order to establish the fact that in each of these regions the solid syphilitic effusion will be found to take place in different degrees of extent and density. These differences depend upon the amount of loose subcutaneous cellular membrane existing in the different regions referred to. The greatest amount of areolar tissue will be found between the external and internal layers of the prepuce. It exists abundantly but in less degree on the body of the penis and about the frenum, while it is nearly deficient on the surface of the glans and at the orifice of the urethra. The more cellular membrane, the more solid deposit. Its abundance favours, while its deficiency resists, the induration which is so characteristic of syphilitic inoculation. Thus we shall find that sores on

either surface of the prepuce are accompanied with the greatest amount of induration. It is often the first indication to the patient of the mischief he has incurred, the original lesion being so minute that it has escaped his notice, and is no longer to be traced by the surgeon. It sometimes forms a dense solid ring round the margin of the prepuce, causing phimosis and consequent subpreputial mischief. The solid deposit is sometimes found in the shape of a large tubercle, which is very difficult to get rid of, and does not become absorbed until after a long period.

Sores on the body of the penis hold an intermediate character as regards solid deposit. The induration beneath the sore has but little thickness, and is best appreciated by raising the ulcer between the thumb and finger. I believe it has been called the parchment sore, and its subsequent cicatrix is raised and dense.

As regards syphilitic lesions involving the frenum the amount of induration will mainly depend on the quantity and looseness of the cellular membrane which varies in different individuals.

Sores incurred on the surface of the glans and around the orifice of the urethra are accompanied by the smallest amount of solid deposit, the induration being hardly appreciable, although it sometimes forms a delicate but very characteristic ring around the lips of the urethra.

The law which I have endeavoured to establish, viz., that syphilitic solid deposit mainly depends on loose areolar tissue, is well exemplified in those sores which so frequently occur in the coronal groove, involving at once the root of the prepuce and the base of the glans. There is abundance of solid effusion on the former, but little or none on the latter.

I have thought it worth while to make this record of my experience, to show that the amount of solid effusion attending syphilitic sores has no relation to the virulence of the poison contaminating the blood nor to the severity and extent of the expected constitutional affections. Indeed, I shall endeavour to prove that the one is in reverse relation to the other, and also that, from the situation of the primary sore, a fairly reliable prognosis may be established as regards the nature and extent of the "secondary symptoms" to be apprehended, enabling us to prophesy as to the results and to treat the case with increased confidence.

Patients are eager to know what they may expect, and it is often most important that we should be able to answer their inquiries.

The reliance which has been placed on the presence, the extent, and density of the solid deposit accompanying or following syphilitic sores has led to much confusion as regards the distinction generally adopted between hard and soft, simple

and specific ulcers, and tends to obscure and mystify our opinion on the nature of the disease and its probable results.

I lay no stress on the size of a sore as having any particular influence on its results, and I must also discard for my present purpose the difference in constitution, temperament, habits of life, and other circumstances, which will necessarily modify primary appearances and "secondary symptoms," and also the treatment to be employed.

After comparing the records of a vast number of complete syphilitic cases, I have come to the conclusion that the seat of the primary ulcer is a most important feature in our diagnosis and our prognosis.

I have already pointed out the different parts of the penis on which sores may occur, and the same arrangement will hold good for our present purpose. I would impress, as a broad rule, that the farther the sore is removed from the solid structures of the organ, that is, the glans and body, the less severe will be the consequent constitutional symptoms. Thus we find that marginal sores, which frequently occur along the extreme edge of the prepuce, although decidedly specific in their character, are seldom or never productive of extensive or severe "secondaries." A few patches of roseola on the body or some scattered lichen will probably be the only result. Not unfrequently even these are wanting, or perhaps a mild specific affection of the fauces will indicate that the poison has reached the blood.

2nd. Syphilitic sores affecting the internal or external prepuce may be considered more serious in proportion as their position is farther removed from the preputial margin, and nearer to the base of the glans and the main body of the penis.

3rd. Sores occurring on the body of the penis are more formidable than the two previous classes, and I have reason to believe that the nearer they are to the pubes the more may be apprehended.

4th. Sores on the glans, more especially when situated at its base, involving the coronal groove and corresponding portion of the root of the prepuce, are most to be dreaded. They almost inevitably, even in spite of early treatment, are followed by widely diffused and long-continued constitutional symptoms, frequently baffling the skill of the surgeon and wearing out the patience of the sufferer.

5th. Sores contracted on the frenum become more or less important according to their extent. When they destroy the entire fold of the frenum, and involve its attachment to the glans and prepuce, they may be classed as sores of the coronal groove.—*Guy's Hospital Reports*, 1877, *p*. 1.

70.—ON THE TREATMENT OF SYPHILIS.

By JAMES R. LANE, Esq., F.R.C.S., Surgeon to St. Mary's Hospital, Consulting Surgeon to the London Lock Hospital.

When I first became connected with the Lock Hospital, thirty years ago, it was the custom to give a course of mercury in every case of primary sore, whether indurated or not. This was done with a view to diminish the liability to secondary affections, although it was generally understood that the healing of the non-indurated sore was in no way accelerated by its use. Since then we have had the demonstration that the soft sore is only exceptionally followed by secondary consequences, and it has therefore become the rule to abstain from giving mercury in such cases. This is, in my opinion, the greatest improvement in the treatment of the primary form of the disease which has been introduced in recent times. It has saved the great majority of sufferers from the evils of an unnecessary course of mercury, with no very appreciable damage to the small number in whom secondary affections occur.

With respect to the treatment of the indurated primary sore modern opinion is divided. There are many good authorities who think that it is better to abstain from mercury in this stage, and to wait until the secondary affections present themselves. They argue that secondary disease is certain to occur, and that it is not prevented but only retarded by mercurial treatment. I cannot agree with this view, for, according to my experience, after-consequences may often be prevented altogether by resorting to mercury as soon as the primary induration becomes pronounced, and by persisting in it till it has completely passed away. If we are right in believing that the induration is an indication that the system is already contaminated, it must be right to attack it at the outset with the most efficient remedy we possess, and it is reasonable to expect that the disease may often be thus prevented from passing beyond its initial stage. All attempts to destroy the primary sore at an early period by cauterisation should be confined to non-indurated sores—in them it will often be successful, and can do no harm; but cauterisation and even free excision is useless after induration has been established. The induration will, after either method, as I have repeatedly seen, return before the wound heals, and the progress of the disease be in no way interrupted.

Of the value of mercury in the treatment of secondary affections I need hardly speak. Its efficacy is now almost universally admitted in the removal of existing symptoms, in hastening the progress of the disease towards a favourable termination, and, where important structures are attacked, as in iritis, in the prompt arrest of morbid processes, which with-

out its aid would often be rapidly destructive. Mercury is pre-eminently the remedy for syphilis in the secondary stage, if used within proper limits, and with the precautions which fortunately are now very generally understood. When the secondary stage has been gone through, and when tertiary symptoms supervene, mercury, as a rule, is no longer beneficial. It was through its persistent use in this stage that it became from time to time discredited, and many were led to ascribe the occurrence of destructive ulceration and bone disease to its malignant influence.

Fortunately, in the compounds of iodine a remedy has been found which is as efficacious in the tertiary stage as mercury is in the secondary. Iodine, in a pure state, in tincture, or in the form of burnt sponge, was used as long ago as 1820 in the treatment of syphilis, but without any striking result. It was in 1831 that Dr. Robert Williams first used the iodide of potassium, in St. Thomas's Hospital, in cases of periosteal nodes, in which, as in other forms of bone disease and of destructive ulceration, the unsatisfactory results of mercurial treatment had begun to be generally acknowledged. The result in Dr. Williams's cases was that with which we are now so familiar: complete relief from pain in a few days, and gradual disappearance of the nodes. He soon after used it in rupia and in tertiary ulceration of the throat, which, as we now well know, heal so marvellously and rapidly under its influence. Iodide of potassium was soon afterwards taken up by Dr. Wallace, of Dublin, who used it in a large number of cases, and published the results in the Lancet for 1836. It was adopted about the same time by Ricord, and its value was soon fully appreciated by the profession.

The beneficial effect of the compounds of iodine (iodide of potassium, sodium, or ammonium) seems to be confined almost entirely to the tertiary stage. I believe that in the earlier secondary period, at all events, it has no influence whatever. The marked difference in the effect of the two remedies in the two stages affords further proof of the striking change which takes place as the disease progresses, in the constitution of the patient and in the character of the symptoms. It is, I know, a common custom to give the iodides in secondary disease as a substitute for, or in combination with, mercury. I do not think the practice is to be commended; uncombined with mercury it does no good; combined it is more likely to interfere with the mercurial action than to assist it.

It is held by many good authorities that the beneficial action of the iodides in tertiary syphilis is confined to the removal of existing symptoms and to the restoration of the health of the patient from the cachectic condition into which it has so often

fallen, but that it does not cure the disease, mercury being afterwards required for that purpose. What I have seen would not lead me to favour this view; there may be occasionally cases of tertiary disease in its earlier stage in which mercury is beneficial, but in the great majority it is positively injurious. It is in the nature of tertiary syphilis to relapse, and I doubt much whether the relapses are rendered less frequent or less severe by mercury. I have long acted, in this stage, on the principle of more syphilis more iodide, and in the end, even in the severest cases, perfect and permanent recovery has often been the result. Neither am I disposed to favour the very common practice of giving mercury and the iodides in combination. I think it is better to act on the principle that when one is required the other is likely to be prejudicial, and that when they are used in combination the tendency of each is to counteract the other. The only cases which seem to call for the combination are those which are in the transition period, and in which secondary and tertiary symptoms coexist. Here it seems rational to combine the two remedies, or to alternate them according to the rise or fall of either set of symptoms, and this, I think, may often be done with very satisfactory results. —*Lancet, June* 30, 1877, *p*. 931.

71.—ON SYPHILITIC REINFECTION.

By George Green Gascoyen, Esq., F.R.C.S., Surgeon to the Lock Hospital, Assistant Surgeon to St. Mary's Hospital.

It was long believed that a person who had once been the subject of syphilis was thereby rendered more prone to contract again this disease, and that if subsequently exposed to contagion he would be liable to acquire one attack of syphilis after another, It was further considered that with each new infection fresh poison was absorbed into the system, adding to the virulence of that already lurking there, so as to stimulate and intensify the existing disease.

Such were the current opinions until Ricord taught that syphilis, like small-pox and other disorders of this class, exerts such an effect upon the economy as to protect it from further contamination. This condition he described as a *diathesis* which, as long as it lasts, confers complete immunity from another infection, and renders the person incapable of developing either an indurated chancre or other constitutional symptom. In his experience this protective influence terminates only with the life of the patient, but he admits that by analogy the syphilitic diathesis should in certain subjects wear out and become extinguished, so as to permit a repetition of the disease, just as recurrences of measles, scarlet fever, &c., are occasion-

ally, though exceptionally, met with. At that date, however, he had never known an instance of syphilitic reinfection, and stated that "science does not yet possess a single well-proved example of a second attack of syphilis."

In consequence of this statement Ricord has been credited with the dogma that a second infection of syphilis is impossible; but, far from this being the case, he distinctly says—"It is not that I deny the possibility of a repetition of the indurated chancre; on the contrary, *I believe in it,* and I believe in it firmly, although clinical experience has as yet refused me proofs of it."

The sagacity of Ricord's perceptions on this subject have now received ample confirmation, not only as regards the persistence of the syphilitic diathesis, which generally lasts for many years, and often, indeed, until the end of life; but also as to the occasional extinction of this peculiar influence, so that the patient may regain his former aptitude to contract the disease, or it has been conclusively demonstrated that syphilis may be repeated in the same person.

The instances recorded amount to a considerable number, but they are mostly scattered throughout the medical periodicals in such a manner as to render them unavailable for general use, and hence they are scarcely known to the profession. I have therefore appended a list of the cases that I have been able to find, with their references, and now proceed to give the details of eleven others which have passed under my own observation.

[We have not room for all these cases but give a few.]

Case 1.—November 7th, 1863. A gentlemen, æt. 28, in good health, consulted me for a large typically indurated chancre on the corona and glands, with indurated inguinal glands. He had a prejudice against mercury and would not take it for five or six weeks, when, the chancre having increased to nearly the size of a florin and showing no disposition to heal, he commenced a mercurial treatment. In a week the improvement was so marked that all reluctance ceased, and he continued to take the medicine for three or four months, until the induration had quite disappeared. Except a slight sore throat of a doubtful character, there were no constitutional symptoms.

On November 16th, 1867, he showed me an abrasion with a very suspicious base on the inside of the prepuce, the result of connection a week ago, which developed into an indurated chancre; two other sores also formed and indurated. The inguinal glands could be felt separate and slightly enlarged, but they could not be distinctly made out owing to the large amount of fat in the groins. The sores soon healed under mercury, but the induration did not pass away until the end of February. On May 25th a large mass of induration had formed

at the frænum, and gradually extended half-way around the corona and glans. Much deposit of similar nature had also taken place in the lymphatics on the dorsum of the penis, quite to its root; these hardenings gradually yielded to mercury. Subsequently (October 9th) a gumma formed on the inside of the right leg, which broke, leaving a very obstinate sore; another appeared, but subsided. A few months after this a tertiary ulcer occurred on the leg, and six months later a deposit again took place in the lymphatics of the penis; this symptom was very persistent, and recurred two or three times. His health throughout was good. He has since remained free from disease, and is now the father of two healthy children.

Case 2.—February 16th, 1864. A farmer, æt. 36, of robust health, covered with a copious eruption of roseola and lichen, following a sore at the corona which is now healed, though a slight induration can still be felt. Under mercurial treatment all symptoms disappeared, and he remained free from disease until—November 12th, 1870, when he came to me with a severe eruption of roseola and psoriasis, and mucous papules of the throat. He said that after intercourse in July a sore formed on the inside of the foreskin, which healed without treatment about the end of August. The eruption began to appear three weeks ago. Under mercury these symptoms passed away, but he had several relapses in the form of psoriasis, mucous papules of the throat and anus, superficial ulcerations on the trunk, scrotum, &c., which required treatment at intervals until the summer of 1873. He has since continued well.

Case 3.—June, 1864. A healthy man, æt. 22, contracted a chancre three months ago, which has healed; has now severely ulcerated mucous papules of the throat, fauces, and palate, very extensive and very persistent, with a fading eruption of roseola and psoriasis. He recovered under mercurial treatment, and remained free from all trace of disease until—

May 9th, 1873, when he showed me three indurated chancres on the inside of the foreskin, the result of connection about a fortnight since. Each sore was situated on a well-defined induration, and could be picked up between the fingers as a small button-like mass. The inguinal glands were not affected. Mercury was given. On the 24th the sores were nearly well, but the induration was still quite perceptible. I have not heard of him since, though he promised to inform me if anything else occurred.

Case 4.—January, 1865. A healthy man, æt. 34. General roseola, with universal enlargement of glands, the result of infection probably in August 1864. There had been great pain and aching in the limbs previous to the appearance of eruption. He had had no treatment. In April he had very severe psoriasis

of the trunk and limbs, followed by double iritis and great impairment of health. He now commenced mercury, but took it very irregularly. He continued in a cachectic state, having at intervals double orchitis, ulcers of the leg, and nodes, until 1870, since when there have been no visible manifestations of the disease; his health, however, has remained much enfeebled, and he has suffered from a lung affection considered to be of syphilitic origin.

In February, 1873, a large indurated chancre formed on the left thumb; it commenced as a pustule, and was contracted on January 24th from a patient who had two suppurating indurated sores on the foreskin, which were followed by roseola. The induration on the thumb was well marked, and the lymphatics over the metacarpal bone became swollen, hard, and tender on pressure, but neither the epitrochlear nor the axillary glands were affected. During March there was much nocturnal aching of the legs, but no other symptoms. No treatment.

Remarks.—These cases of syphilitic reinfection are of much practical value in showing that the effects of syphilis upon the system are not of necessity life-long; but that the diathesis, or *intoxication*, as some prefer to call it, created by the disease may gradually wear out and become extinct so as to allow another infection. For we cannot admit that a person already under the influence of syphilis can suffer another general contamination, and thus become the subject of two concurrent attacks of the same disease, each of which must be passing through a different stage of development.

The possibility of reinfection proves also that a syphilitic person may entirely regain his former condition of health, and, therefore, become the father of healthy offspring. Thus is explained a circumstance with which we have long been familiar, that persons whom we know to have been the subjects of syphilis will apparently recover from it, remain free from recurrence for the rest of life, and beget healthy children without a trace of hereditary disease. Such instances, and happily they are not rare, would lead to the conjecture that the disease cannot be merely in abeyance, and biding its time for a fitting opportunity to again break forth, but that it has been completely got rid of; and the fact that a second infection is possible proves that such may really be the case. Nor would it seem that instances of recovery from syphilis are very uncommon, for we have clinical evidence of it in such observations as those just alluded to, whilst the published cases of a second contagion now amount to a considerable number.

These cases of syphilitic reinfection bear also with peculiar significance upon a much debated, though most important, question—viz. *the curability of syphilis.* In the instances here

related, as well as in most of those recorded by others, a full treatment by mercury had been adopted, thus affording, should it still be really needed, additional testimony to the therapeutic value of that drug in syphilis, and confirming the opinion of those who state, as the result of long experience, that a prolonged course of mercury is the most certain method of preventing the more severe, as well as the more remote, lesions of syphilis. Great encouragement is, therefore, given to persevere in this mode of treatment, inasmuch as it offers a reasonable hope of *curing* the disease.

Many other points of interest are opened by these cases, to some of which I will refer, though they have but an indirect bearing upon our present subject.

They seem to throw much light on the real import of the induration which usually accompanies an infecting chancre, and is regarded as a necessary part of primary syphilitic ulceration rather than as one of the manifestations of general syphilis. When inoculation of an indurated chancre succeeds upon a person already the subject of syphilis, an ulcer with a *soft* base is produced, which in no respect differs from an ordinary non-indurated chancre, either in its appearance or behaviour; but, although no induration attends this sore, its real nature is in no respect altered, for when transmitted to a healthy individual a chancre forms which becomes indurated and gives rise to constitutional disease. Here, then, we see that although an infected person cannot escape the local action of the syphilitic poison—as evidenced by the formation of an ulcer—yet no induration is produced by it; a circumstance which, to my mind, shows conclusively that this symptom is not one of the local processes essential to the development of a particular form of chancre, but that it entirely depends upon a constitutional cause, which is unable to manifest itself as usual at the point of inoculation, in consequence of the economy being already under the influence of a former contamination.

The presence, then, of induration about a sore shows in the first place that the system was free from a previous syphilitic taint; and, in the second, that the patient is under the influence of a general syphilis of recent date; so that the formation of an indurated chancre in a person who has formerly had syphilis proves that he must have completely recovered from the foregoing attack.

Again, that induration is a constitutional symptom, and not a local phenomenon, is seen in the fact that a person who has an indurated sore, but is as yet without any other manifestation of syphilis, will, if exposed to contagion, contract a chancre with a soft base, but not another indurated sore; and the same result obtains from the spontaneous or artificial auto-inoculation of the original chancre.

I claim, then, on the above grounds, that the induration at the base of a chancre is as certain evidence of a general contamination as any of the recognised secondary symptoms, and that when a chancre has become indurated it has ceased to be a primary affection.

Another thing which may be learnt from the cases just related is that a period of incubation does not necessarily precede the evolution of an indurated sore, for in most of them the chancre which initiated the second disease followed directly upon intercourse. This shows, I think, that the contagion must have been acquired from a primary sore which was still suppurating and in full activity, rather than from an indurated chancre which had ceased to suppurate, or from that very common cause of infection—mucous tubercles; in other words, that the disease must have been received from a *primary*, and not from a *secondary*, lesion of syphilis.

As long as the local processes are active about a chancre and it continues to suppurate, it is highly virulent and capable of ready inoculation, producing at once a pustule, succeeded by an ulcer, which in the case of the infecting chancre usually begins to indurate about the fifth to the eighth day. The inguinal glands take on a similar action about the same time, and as the chancre becomes indurated it passes into an indolent condition and ceases to form pus.

When the serous secretion of such a sore is inoculated it no longer gives rise to the same phenomena as those which follow the insertion of chancrous pus, but they are so precisely analogous to those which have been observed to attend the inoculation of mucous tubercles or the blood of a syphilitic person, by direct experiment, that the same description will suffice. In both, a little reddening follows the puncture, which soon dies away, but after a period varying from three to six weeks a hardened papule begins to form at the point of insertion; this slowly enlarges, desquamates on its surface, and finally ulcerates. The glands also become simultaneously affected.

Both these modes of development, which have been seen to attend artificial inoculation, are met with in practice as the result of physiological contact. In some cases the sore follows immediately after connection, whilst in others there is a delay of varying length, known as the *period of incubation*, before the formation of the ulcer. When incubation occurs it shows that the infection has been received from some constitutional lesion of syphilis, or from an indolent non-suppurating indurated sore, which, as before stated, I look upon as a truly secondary affection. When, however, the chancre follows at once upon intercourse, its origin may be assigned to an active suppurating sore of recent date—*a true primary chancre*.

In the one instance there is afforded an illustration of *secondary*, and in the other of *primary* syphilitic inoculation by natural means; and the resulting chancres at first differ as much in their appearance and conduct as in their mode of evolution, although the sore which occurs from primary inoculation may gradually lose its characteristics, and, becoming indolent, assume those of the chancre which is produced by the inoculation of secondary syphilis.

The propagation of syphilis is, I believe, greatly due to contagion imparted during its secondary stage, and hence the frequency with which examples of secondary inoculation—those in which a period of incubation is present—are met; so much so, indeed, that some modern authors altogether deny the *début* of syphilis by a pustule. The older writers, however, speak of this as the usual commencement of syphilis, and certainly clinical observation will lend testimony to its occurrence in a number of cases. A primary chancre gives rise to pain, and attracts the attention of the patient, who perforce abstains from intercourse for a time, and seeks relief for it. The secondary contagious affections, such as mucous tubercles, on the other hand, are generally painless and often pass unnoticed, so that the person continues to spread disease in ignorance of his dangerous condition.

The syphilitic poison loses in virulence and in its ready inoculability with the lapse of time, until at length a period arrives when it ceases to be communicable. The secretion, therefore, of an active primary sore is more powerfully contagious than that of a secondary affection, whilst that of the more advanced lesions is altogether incapable of inoculation. It would seem also that persons who have had syphilis, instead of being more liable to contract again this disease, as was formerly believed, are in reality less so; their receptivity apparently having been diminished by the former attack, and a stronger virus may therefore be required to infect such individuals than in the case of those who have never suffered, and who are consequently susceptible to the influence of a weaker poison. A person who has not had syphilis may, therefore, become contaminated by a secondary accident which would prove powerless to infect one who has had the disease; and thus, perhaps, a partial explanation may be afforded why in these examples of syphilitic reinfection a period of incubation does not more often precede the development of the chancre, since it is not improbable that the contagion has been acquired from a primary syphilitic ulcer, in which case the sore would follow immediately upon intercourse.

The comparative rarity of cases of second infection may perhaps to some extent be referable to the stronger virus of a

primary chancre being necessary to excite a general reaction when the feebler virus of mucous tubercles would fail to do so; or it may be that the secretion of these latter is too weak to produce more in such individuals than a slight manifestation at the point of insertion which is overlooked.

The reason why in so many instances the disease has been limited to an indurated chancre may possibly be that, although the former diathesis was sufficiently extinguished to allow another infection to take place, yet the system had not regained its normal condition sufficiently to permit an outbreak of general disease, and consequently the effect of the poison was limited to ulceration at the point of insertion, about which induration, the first and slightest of the constitutional symptoms, was developed. But that these chancres, even when they do not give rise to any other symptom than induration, are powerfully infecting and capable of causing severe disease in a healthy subject, has been shown by Diday in a case which occurred in his own practice; and it has further been proved that such chancres have been derived from specially indurated sores which were followed by constitutional disease in the persons transmitting them.

Some of these cases of second contagion aid also in determining the position which certain of the manifestations known as *tertiary* actually hold in the *rôle* of syphilis; whether the more advanced of them are really due to the persistence of the disease and its still active presence in the system, or whether, with Mr. Hutchinson, we are to regard them as evidences of injury inflicted upon the economy by a disease which has now come to an end.

This latter view of the subject receives strong support from the circumstance that the more remote affections are, so far as we know, incapable of transmitting the disease from which they originated, either by contact, inoculation, or inheritance; and further confirmation is also afforded by some of these instances of reinfection, for no less than six of them occurred in persons who were suffering at the time from tertiary syphilis. Unless, then, it be conceded that two distinct attacks of general syphilis can co-exist in the same individual, we must conclude that the so-called tertiary accidents which were present when the re-infection took place, are merely the tokens of a bygone malady, and not the evidences of an existent disease.

But, whilst admitting that certain of the symptoms now classed as tertiary are merely the consequences of a lapsed syphilis, I cannot doubt that the earlier of them should be placed to the active operation of the syphilitic poison. It is true that they occur at a time when the economy has been subjected to the lowering influence of syphilis for a lengthened

period, and possibly the peculiar forms they assume may be determined by the constitutional impairment, and so modified by it as to acquire the characteristics of those idiopathic affections which result from depraved health or defective vitality, however occasioned; but at first they so generally respond to the influence of special remedies that I am compelled to believe that they must be due to the continuous working of the disease in the system. A time, however, comes at length—marked by grave cachexia, unhealthy plastic deposits, and intractable ulcerations—when specific treatment ceases to be of service, and and when tonics, liberal diet, and general care prove far more beneficial than special medicines; when this condition is reached the lesions attending it do not, I think, result from a still active syphilis, but from the degradation which the blood and tissues of the body have suffered in consequence of its previous effects.

I must apologise to the Society for the lengthy digressions in this communication, but the cases seemed to lead to the consideration of certain points still unsettled, the interest and importance of which must be my excuse for having introduced them.—*Medico-Chirurgical Transactions*, vol. liii., *p.* 7.

72.—ABSTRACT OF CLINICAL LECTURE ON SYPHILIS.

By S. M. Bradley, Esq., Surgeon to the Manchester Royal Infirmary, and Lecturer on Practical Surgery, Owens College.

Definition.—Syphilis is an acquired, or hereditary, specific contagious disease, characterised by local manifestations at the seat of infection, and followed by general symptoms of a slow, inflammatory, progressive kind, invading first the skin and mucous membrane, and subsequently every tissue of the body.

The above may serve as a definition, but requires the amplification given in the annexed sketch of the periods of syphilis, which affords a sort of glance at the entire history of the disease.

Sketch of the Syphilitic Periods. — Syphilis is probably a zymotic disease, and like other zymotic diseases propagates itself by contagion; the contagium particles being only capable of transmission by direct inoculation from one animal to another. The poison (zymes, or germs) having penetrated beneath the cuticle, lies dormant for a varying period (*period of incubation*), before it gives rise to the local manifestations in the shape of an ulcer or papule (*period of primary symptoms*). This ulcer, unless suppurating so freely as to directly discharge the poison germs, communicates the contagion along the lymphatics to the nearest gland, which on the receipt becomes inflamed.

Here, again, a final arrest of the poison may take place by

the gland suppurating, and thus throwing off the virus; or, after a time, during which the germs are travelling from one gland to another, the entire chain is traversed, and the germs, no longer impeded by gland structure, pour themselves along the thoracic duct into the blood current, their entrance being ushered in by pyrexia (*period of syphilitie fever*), and speedily followed by a cutaneous and mucous eruption (*period of secondary symptoms*). At this stage the disease may be eradicated, or it may proceed until the various fluids and tissues of the body are each in turn affected by the virus. After a time, greatly varying in duration, the virus becomes somewhat changed in character, no longer giving rise to distinctly inflammatory symptoms, but being poured out as a diffuse, or circumscribed, gummy exudation, into the various tissues and viscera, the exudation showing little tendency to suppurate or undergo any organisation higher than that of cellular tissue (*period of tertiary symptoms*). At this stage, the disease may be finally arrested, or may continue with only trifling modifications for the rest of the patient's life.

Varieties of Sore.—1. Soft chancre. 2. Dry Papule. 3. Chancrous Erosion. 4. Indurated chancre. 5. Mixed Chancre.

Description of the various kinds of local sore:—

Soft Chancre—Synonyms—*Non-Infecting Chancre*—*Local Contagious Sore*—*Suppurating Chancre.*

1. The *Soft Chancre* has a crescentic or circular outline, freely suppurates, and is more or less deeply excavated. If the chancre is extending, the edges are undermined; if stationary, the edges are clean cut. The base is always soft. This sore appears as a rule within a week after infection, and generally lasts about a month. It may heal more quickly or may resist treatment, either remaining stationary, or spread sometimes slowly, at others rapidly by phagedæna, until months and even years are occupied in fruitless efforts to heal the ulcer. The pus from a soft chancre is auto-inoculable. This sore is often followed by a bubo in the groin, as a rule only one gland being affected. This glandular affection is sometimes merely sympathetic, a simple adenitis ensuing which may or may not end in suppuration, and sometimes is due to direct conveyance of the virus along the lymphatics to the gland, in which case the gland nearly always suppurates, the resulting sore resembling the primary ulcer, and being like it auto-inoculable. The soft chancre varies in size from a split pea to a broad bean, or larger; it is often multiple, and is very rarely followed by secondary manifestations.

2. *The Dry Papule* is a rare form of primary lesion. It is a slightly raised brownish-red papule covered with fine scales, and very much resembles a secondary syphilitic tubercle. It

has generally a lengthened incubation of from thirty to fifty days. This sore is associated with basal induration terminating abruptly in the surrounding tissues. The average duration of the dry papule is two months; it is always single, and is almost invariably followed by constitutional infection.

3. *The Chancrous Erosion*—Synonyms—*Patchy Excoriation—Superficial Primary Syphilis—Chancriform Erosion.*—This lesion, which is very common, appears in the form of an abraded sore either level into the surrounding surface, or slightly raised by exudation; it is generally small and somewhat circular, at times, however, quite irregular; it secretes a little serum, but no pus, and after a few days the base becomes indurated, and the glands in one groin hard and rather tender to the touch. The average period of incubation is 21 days, but it may appear at any date from the tenth to the ninetieth day after infection. The chancrous erosion is rarely dual, and never multiple; it heals in about a month, the basal induration lasting some two or three months longer, and is almost invariably followed by secondary symptoms.

4. *The Indurated Chancre*—Synonyms—*Hunterian Chancre—Infecting Chancre—Non-Suppurating Sore.*—This sore, which is about equally common with the chancrous erosion, is circular, elliptic, or irregular in shape, and varies in size from a split pea to a broad bean. It is perceptibly excavated, with hard edges, and has a cartilaginous base which extends beyond the area of the ulcer, and can be lifted up from the environing tissues. This typical syphilitic sore does not commence as an ulcer, but as a small papule, which subsequently ulcerates. The surface is glazed, or covered with a slight secretion of serum and tissue *débris*. The glands in one groin become enlarged before the basal induration disappears, and following this we almost always have secondary manifestations. It, like the former sore, has a widely varying period of incubation; the average, however, is about three weeks. It lasts as a rule about three months, but the induration continues longer, the resulting cicatrix at times remaining perceptibly indurated throughout the rest of life. The indurated chancre is rarely dual, never multiple.

5. *Mixed Sore.*—Synonyms — *Chancre first soft and subsequently indurated.*—This form of sore must not be overlooked; commencing as a soft chancre, induration subsequently appears, often during the process of repair, and the subsequent local and general history is identical with that of the chancrous erosion.

These, then, are the various forms of primary lesion; but before passing on it is necessary to say a word about chancres in the urethra and chancres in the female.

Chancres in the Urethra.—Although chancres may be met

with in almost every part of the body—*e.g.*, I have seen one above the eyebrow—urethral chancres require special mention, because they are especially prone to be overlooked. Chancres in the urethra may be either of the soft or hard varieties, and generally occur within the first half-inch of the passage, though they may be placed much lower down, even as far back as four inches from the meatus. The symptoms vary as the sore varies; thus in case of the soft chancre, we have a puriform discharge, sometimes abundant enough to closely simulate a gonorrhœa; while in case of the hard chancre, the discharge is scanty and sanious, at times being scarcely perceptible. Both sores are painful to a certain extent when pressed between the finger and thumb, and this means of examination should always be carefully employed in determining the cause and nature of a urethral discharge. If a soft chancre is present, the hollow ulcer and the ragged edges can generally be made out; and if it be a hard sore, the diagnosis is even more easy and certain from the distinct nodule of indurated tissue around it. The endoscope might be useful in doubtful cases.

Chancres in the Female require more careful search than chancres in the male. Their common situation is the nymphæ and neighbourhood, but the vagina must always be thoroughly examined, as they not only lurk here at times, but are even met with upon and within the os uteri; the lips and mouth too should be inspected in doubtful cases. It is almost certain that the female is liable to every form of sore which infects the male, but as yet, I believe, there is no recorded case of the dry papule having been met with as a distinct primary lesion.

Period of Incubation.—The period which elapses between the reception of the poison and the appearance of the local lesion is termed the period of incubation. As may be surmised from what has gone before, this period varies greatly, ranging from two to ninety days. The soft sore has a shorter period of incubation than any of the more generally infecting varieties of chancre. From five to ten days is the average period of incubation for the soft sore; from seventeen to thirty—twenty-one being the mean—is the average for the hard sore.

There are, however, many well authenticated cases on record of much longer periods elapsing, histories being recorded of fifty, sixty, seventy, and even ninety days intervening between the date of infection and the appearance of the ulcer; in these cases as a rule the constitutional symptoms are general and severe.

Inoculability.—The property of inoculability upon the same subject is common to all forms of the syphilitic sore, but in very different degrees; the soft sore being invariably capable of auto-inoculation, while the other kinds of primary lesion are very rarely auto-inoculable. Successful attempts at auto-inoculation

have been made, however, with the indurated chancre and with the chancrous erosion, and it now appears probable that the entire difference depends upon the presence or absence of pus. When pus is present, as is always the case with the soft sore, auto-inoculability is the rule; when pus is absent, as is generally the case with the hard sores, auto-inoculability is the exception. Moreover, as we shall see in the next paragraph when we discuss the unity of the syphilitic virus, this question of auto-inoculation is no longer so important as it was deemed when the doctrine was held that the soft auto-inoculable sore was purely a local lesion quite distinct from syphilis, and that the use of the term, syphilis, should be confined to the non-auto-inoculable sores; viz., to the dry papule, the chancrous erosion, and the hard chancre.

Unity of Poison.—With the exception of the mixed chancre, every form of syphilitic primary lesion as a rule propagates its kind; this is especially so in the case of the soft and the hard chancres, the dry papule and the chancrous erosion, though often the direct offspring of similar lesions, are not unfrequently transmitted from certain secondary affections; *e.g.*, condylomata.

This circumstance, coupled with the fact that the soft chancre rarely infects the system, while the hard sores are generally followed by secondary symptoms, led observers to doubt the unity of the poison, some even maintaining that there was a distinct virus for each kind of sore, while most went so far as to say that there were certainly two distinct poisons; the one giving rise to the soft chancre, which was not a poison which invaded the system beyond the first inguinal gland, the other the parent of the various indurated sores which always infected the constitution at large. As knowledge ripened and syphilographers multiplied, this doctrine proved difficult to hold, if not untenable; cases occurred where sores pronounced soft chancres by competent judges were followed by constitutional symptoms; instances of the auto-inoculability of the hard sore were from time to time reported, and in spite of ingenious theories that in such cases as these we had a dual poison to deal with, grave doubts were thrown upon the validity of the doctrine of duality, doubts which became clearly manifest in the evidence given before the Admiralty Commission in 1870.

It was, I believe, my lot to reduce this vexed question to a demonstration, and prove that there is an essential unity in the syphilitic poison; for in some experiments, published in the year 1872, I succeeded in producing an auto-inoculable sore from a typical indurated chancre, and again in obtaining constitutional infection by inoculation from a freely suppurating non-indurated sore. Still though "duality is dead," it yet

speaks, and furnishes us with useful hints in both prognosis and treatment. It is the very general rule for the syphilitic primary sores to breed true; the soft sore produces the soft sore, and as the virus is wholly thrown off in pus, requires no mercurial treatment from first to last; the hard sore gives rise to the hard sore which infects the entire economy, and requires a lengthened mercurial treatment to remove its eradication from the system. —*Medical Press and Circular, June* 27, 1877, *p*. 505.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN

73.—ON MORPHIA AS A PARTURIFACIENT.

By Arthur Wiglesworth, Esq., L.R.C.P., Liverpool.

The principal portion of the muscular substance of the uterine body is composed of unstriated fibres, arranged in three layers—the internal, which are circular; middle, mostly longitudinal; and external, which are chiefly accessory to the middle layer. But in the cervical portion of the uterus they are somewhat differently arranged. There the circular fibres form the largest layer, and are in the middle, and form the so-called internal and external sphincters of the uterus; whilst the longitudinal are placed externally and internally, interweaving with the circular bands at the os externum and internum. (Stricker, vol. iii. p. 476.)

The os externum and internum, or the external and internal sphincters of the unimpregnated uterus are, however, in a somewhat elementary condition, very different indeed to what we find in the gravid uterus at full term. Then we find a powerful sphincter muscle composed of the unstriated muscular fibre, uncontrolled by the will, and retaining the whole contents of the uterus, acting in a similar manner to those fibres placed at the pyloric orifice of the stomach—viz., to restrain for the time being the escape of the contents. The powerful nature of the circular fibres of the os uteri is well adapted for the object in view—viz., to retain the whole of the uterine contents within the uterine cavity against all opposing forces, until the term of fœtal life has arrived; then, in accordance with the law of Nature, these fibres relax, to allow the passage of the fœtus and appendages. In a normal condition of labour the cervical circular fibres slowly and gradually relax until full dilatation is accomplished, and the passage of the head and body takes place. But in certain cases a different condition of things exists. After the os uteri has opened to an extent varying in size in different cases, it fails to dilate further, and, notwithstanding the strongest uterine contractions—notwithstanding the most forcible expulsive efforts of our patient—it refuses to dilate beyond this point, and the natural progress of the labour is consequently delayed.

It is to this condition of the os uteri—called a "rigid os,"

"whipcord os," "undilatable os"—that I am desirous of calling attention; and I believe, and the object of this paper is to show, that this condition may be dependent upon two causes—one direct, the other indirect, or immediate and remote; the first being the result of some morbid state of the "os" itself; the other due to reflex action dependent upon systemic causes. And, further, that both these causes may act in producing a spasmodic condition of the circular fibres of the os uteri, and consequently a retardation of labour; for so long as this spasm exists, progressive dilatation is retarded, unless indeed the system becomes so completely prostrated that there is not sufficient nervous energy left to allow of its continuance.

In order to demonstrate this, I must refer to analogical conditions of the general system of the body which bear upon the point, to illustrate my views.

There is a condition of the vagina termed vaginismus. In this morbid condition the sphincter vagina is in such a state of irritability that it is frequently impossible to introduce either a finger or a speculum into the vagina; and even in milder cases, when either of these may be introduced with comparative ease, yet upon attempting sexual congress, the immediate increased flow of blood to the part, and the nervous excitement consequent upon the act, so increases the spasm, that the natural accomplishment of the act is completely prevented.

Now the cause of vaginismus, as we all know, may be found either locally or systemically. In the first instance the "fons et origo" may be a small fissure or minute tumour, chronic uterine inflammation, or even excessive irritability of the vaginal walls themselves. And it may also be caused systemically by a nervous, irritable, unhealthy condition of the body, or even by direct nervous excitement. Another instance of sphinctal spasm will readily occur to the mind—viz., spasmodic retention of urine. Locally we find gonorrhœa the chief excitant, and systemically the cause is frequently cold, alcoholic abuse, &c. We are also familiar—and many of us practically so—with that condition of the mind which, from nervous agitation or excitement, precludes the act of micturition, and, notwithstanding the most forcible expulsive efforts, the nervous spasm cannot be overcome.

Spasm of the sphincter ani may also serve as an illustration. Fissure or rectal ulcer may be the local excitant, while systemic disturbance *may* also be the cause.

But there are two other apertures also the subject of spasm, which may perhaps be better illustrations to take, as they are both entirely independent of the will. The structure of the pylorus is as nearly as possible analogous to the structure of the cervix uteri at full term as we can imagine. The function

of both is the same—viz., to detain the contents of the respective cavities of which they are the guardians until the proper period arrives, when their circular fibres should relax to allow the contents to pass through the orifices. Now we know that ulceration just at the pyloric orifice of the stomach will frequently cause spasmodic closure of the pylorus and the rejection of gastric contents ; and also that the entrance of unsuitable matters into the stomach will cause contraction of the sphincter, often retaining these matters till rejected by vomiting. And we further know that from systemic irritation, or nervous distress, food that has been partaken of hours previously is retained in the stomach by the action of the pylorus until rejected. I know of an instance in which the rejected contents of the stomach consisted of a perfectly undigested meal partaken of more than twenty-four hours previously. Nothing but a spasmodic condition of the pylorus, induced by Nature herself, could have prevented the gastric contents passing into the duodenum and onwards. The other condition of spasm is closure of the glottis —laryngismus stridulus—a disease essentially of childhood, which is produced by reflex action, induced by derangement of the stomach, bowels, or by the irritation of teething; and occasionally the closure is so completely maintained, notwithstanding the most violent efforts of the powerful inspiratory muscles, that death is the unfortunate result.

These cases then indisputably prove that there exists occasionally a spasmodic condition of the muscular fibres that constitute sphincters, and that this induced spasm may vary in duration, and may be called into activity either by direct irritation or by reflex action induced by systemic or nervous derangement.

If then this effect is produced at every aperture of the body guarded by bands of muscular fibres, I do not see why it may not be produced in the circular fibres of the cervix uteri, provided we have as causes the same or similar excitants. Indeed, considering how plentifully the uterine body and its cervix are supplied with sympathetic nerves, it would be strange indeed if it alone, of all the organs of the body, was to be exempted from spasmodic contraction.

I must here call attention to a statement of that celebrated obstetrician, Dr. Ramsbotham, which is in direct contradiction to the above view, but which I think you will perceive may be explained by the fact that Dr. Ramsbotham has mixed up two classes of rigid os uteri, which I hold as perfectly distinct, requiring totally different treatment. In his work on "Obstetric Medicine" (3rd edit. p. 225) he states that this condition of rigidity does *not* depend upon accidental spasm, but upon "an *originally* firm, hard, rigid, unyielding texture." Strangely,

however, only three pages further on (227) he says: "Under a state of preternatural rigidity of the os uteri, it not unfrequently happens that, without any apparent cause, and independently of any means used, sudden relaxation takes place, and from that time the labour progresses with much greater rapidity." Therefore, in the face of the previous assertion that this condition is *not* dependent upon spasm, we have the admission that the relaxation is sudden in some cases, and the fact of its being sudden shows that it cannot be the *gradual giving way of hard, rigid, unyielding structure.* We must therefore examine this view of the subject for an explanation, and I have no doubt that these opposing statements can be so reconciled as to fully support the views I am endeavouring to enunciate. There is doubtless a condition of the os uteri, most aptly described by Dr. Ramsbotham as a "firm, hard, rigid, unyielding texture," and this condition will under no circumstances give way suddenly, save and except by rupture or incision. It will be found nearly as rigid during the interval of pains as it is during the strongest uterine contraction. The causes of this class of rigidity are, invariably, structural changes, and it is this class that I think Dr. Ramsbotham means when he speaks of a "firm, hard, rigid, unyielding texture;" but in those cases of rigidity which I assert are solely due to spasm, we find a firm, hard, rigid, unyielding condition *only during uterine contraction;* but, when the pain has subsided, we are often able to stretch the os uteri in various directions with either more or less facility. Rigidity or firmness of the os uteri, of course, always exists in a more or less degree during uterine contractions, and is, I opine, a wise provision of Nature, its object being to prevent a too speedy exit of the uterine contents, for it is familiar to all that a speedy completion of labour, from first to last, is liable to be followed more or less by hemorrhage and indirectly by other disasters.

We may have, then, two causes to account for the non-dilatation of the cervix uteri after labour has more or less progressed—one the result of structural change, as inflammation with deposition of plastic material, cancerous deposit, fibroid tumour in cervix, or other interstitial changes; the other, the result of a spasmodic condition induced by either local or systemic disturbance or sympathetic action.

Our inquiry must then be directed with a view of ascertaining what condition, uterine or systemic, can produce such a result as a rigid cervix. Locally, I believe an ulcerated condition of the os, co-existing with pregnancy and parturition, may prove a sufficient excitant of spasm, because as the ulcerated surface becomes involved in the dilatation it must naturally involve also the nerve extremities, which are already in an excited or

unhealthy condition. The same may be said of that fissured condition of the cervix uteri which we so frequently find co-existing with spasm. Vaginitis may also give rise to such a condition, or a sub-acute inflammatory state of os and cervix, and as a fertile source we may have that painful and morbid condition termed irritable uterus, which manifests itself in frequent irritable spasmodic contractions, taking place before the commencement of parturition, days or even weeks. We are often told by our patients they have been in labour for two or three days, and an examination discovers, very frequently, an os uteri dilated to the size of sixpence, and often rigid, this condition being dependent upon an irritable condition of the uterus generally. Any of these conditions (and others probably) may exist, and reasoning by analogical reference to what occurs to other sphincters under similar conditions, we can understand how they may give rise to spasm from direct irritation.

With regard to systemic derangement acting by reflex agency, we might overhaul almost the whole body. Hemorrhoids, overloaded colon, renal mischief, hepatic disorder, derangement of the stomach, cerebral disturbance, mental anxiety, or a highly sensitive condition of the nervous system; each or any of these do produce by reflex agency disturbance of other organs, and, by parity of reasoning, can produce uterine irritation.

But, to my mind, the very remedies that have been advised and used to produce a relaxed condition of the os uteri, entirely favour the view that the condition indicated is purely one of spasm.

Tartar emetic in days gone by was very extensively used, and is to a certain extent now, but by no means with invariable success. But when successful, the result was and is attributed to the depressing effects of constant nausea, just in the same way that before the days of chloroform it was administered to muscular subjects to facilitate reduction of dislocations, by producing depression of the system, and consequently muscular relaxation. It may, however, act beneficially as an emetic, by ridding the stomach of acrid secretions. But it could *not* act beneficially if the rigidity was *not* the result of spasm, but of a "hard, firm, rigid, unyielding structure." Nevertheless, we ought to hesitate before producing debility and exhaustion in a parturient woman.

Bleeding (in days happily gone by, or nearly so) was strongly recommended. A case is on record of a lady being bled to syncope three times, but without the desired effect; and death unfortunately ensued. Nevertheless, bleeding to faintness, or even actual syncope, was regarded as essential, and even in so

late an obstetrical work as Leishman's it is recommended to take from fourteen to sixteen ounces of blood. Taking into consideration, however, as we must always do, the possibility of post-partum hemorrhage, I think the less we have to do with the abstraction of blood in these cases the better, more especially when we also take into our reckoning the possibility of the desired effect not being produced, and further that we thereby give or enlarge an opening for the entrance of metritis, septicæmia, and those other morbid actions which are so liable to ensue upon a long, lingering labour, complicated with exhaustion. When, however, bleeding has proved useful, we must put it down to the well-known effect it has in relaxing spasm.

Tobacco enemata have also been recommended, but considering the doubtful and uncertain effects, combined with the danger that has ensued from their use in other cases, the exhibition of them will hardly be prudent. Here again the desired effect would only be produced by exciting nausea and depression—two things very undesirable to induce under the circumstances.

Rubbing extract of belladona upon the os uteri has also been advised, upon the principle that it aids in relaxing sphincters: also from its peculiar effect upon the ciliary muscle. But its application may prove dangerous, and moreover the effect is doubtful.

The hip bath has also been recommended (M'Clintock, 269), but warm hip baths are not always at hand or convenient.

Chloroform and chloral hydrate have also been used in rigidity, and at first sight we might expect a beneficial effect. But experience teaches us that this is not so invariably; and I look for the explanation of this in the circumstance that if the spasm is severe its relaxation will not be effected unless the administration is carried to its fullest extent, and then if this *is* done there is a great suspension of uterine pains, a not desirable effect.

All these and numerous other remedies have been tried and recommended more or less empirically, and all have more or less failed to fulfil what was required of them, and when successful the desired action would often be in exact ratio to the amount of exhaustion induced, and consequently in their very action undesirable and prejudicial to the speedy recovery of the patient.

I will now mention the drug that forms the subject of this paper—viz., "morphia;" and it would *à priori* appear that morphia was the very drug of all others not likely to be employed when we were desirous of completing a labour with as little delay as possible to ourselves, and with benefit to our patients. Experience, however, will teach us differently, premising, however, that we exhibit it only in suitable cases.

In such cases we need have no fear whatever that labour will be permanently stopped, as stated by Dr. Ramsbotham. In his work, already referred to, p. 225, he says: "The danger of opiates exhibited under labour is, that the uterine contractions may be so entirely removed through their agency *as never again to be established*, and thus the case may be converted into one requiring the use of instruments, perhaps even of a destructive kind." Now, with all due respect to such a great authority, I think practical experience has proved this to be a fallacy. We might as well expect that a full dose of an opiate given in a case of spasmodic retention of urine would so paralyse the bladder that the natural efforts to relieve itself would never return, and the patient be ever after subjected to the use of the catheter. Doubtless if you give a full dose of opium in an essentially natural progressive labour, you may suspend uterine action; but the suspension is only *temporary;* and as surely as the effect of the opiate passes away through the natural channels, so surely and so inevitably will uterine action recommence. It is most unphilosophical to assert that opium in *any* form can permanently suspend uterine action. Nay, I would go further, and assert that if you continued to exhibit this drug in as large doses as would be compatible with the safety of the patient, the uterus, like any other organ of the body would become inured to its presence, and labour would recommence and be completed.

To ascertain the action of morphia in a perfectly natural case of labour, and in a woman whose parturient action was always prompt, I administered one-eighth of a grain of muriate of morphia, when the os was about half dilated, and the result was a cessation of uterine pains for three-quarters of an hour. I then gave a dose of ergot, and labour recommenced. Had I waited until the morphia had been eliminated through the natural channels, the uterus would doubtless have taken upon itself spontaneous action.

If, then, upon examining our patient we find a rigid os uteri depending upon spasm, either direct or induced by reflex agency, if the pains are full, strong and regular, and yet no increase in the dilatation, but a hard constricting cervix with each access of parturient action, then we have a typical case calling for our remedy. If under these circumstances we give our patient from $\frac{1}{8}$ to $\frac{1}{3}$ of a grain of morphia, we shall frequently find that by the time that the morphia has been fully absorbed into the system the constricted os will feel more supple, will gradually relax, and the pains continuing unabated, labour will be completed, provided that no other impediment exists to delivery.

Such a complete change occurring in such a remarkable way, and frequently in a very short space of time, naturally suggests

the looking for an explanation, and the inquiry therefore arises, How does morphia act to produce such results without impairing uterine action? If we take it for granted that the condition of the uterine cervix is dependent upon spasm, then we must necessarily expect that the agent employed will act in such a way that this spasmodic condition shall be overcome or removed, and of all remedial agents none act so efficiently and surely as morphia in removing spasm. We have only to look at the effects obtained when this drug is fully exhibited during the pain and spasm that exists in the passage of biliary or renal calculi, or the relief obtained in spasmodic retention of urine or in colic, to endorse our opinions as to its great efficacy, and this effect is produced by its action upon the muscular fibres of the morbid part, and we regulate our dose in direct ratio to the violence of the spasm and pain. But we find that in the cases under our consideration, although the opiate relieves the rigidity of the muscular fibres of the cervix, nevertheless it does not impair the strength or frequency of the uterine pains. In looking for an explanation of this phenomenon, I think we shall find it in the principle that guides us in the administration of our remedies generally, or rather in their therapeutical effects. For we find that a drug will act upon some morbid process existing in an organ, without interfering with or deranging the natural functions of the organ itself, as it might do if no morbid condition existed. For, if I may so explain it, the drug exhibited has a special function to perform, or a part upon which to expend its virtues, and it only fulfils that function, and acts upon that part.

But if no morbid action be present, then the drug, having no proper destination, acts as a foreign body and produces deleterious effects. In short, the medicament which was beneficial in the first instance becomes prejudicial in the second. Various inflammations, spasms, neuralgias, or other morbid actions in the body, are relieved or cured by remedies acting beneficially upon the parts, either directly or through the nervous centres involved, the other conditions and functions of the body being comparatively unchanged; but prescribe these remedies in healthy subjects and the results are disastrous, and even death might ensue.

Applying this, then, to our subject, we have a spasmodic condition of the os uteri, and upon this morbid condition the opiate exhibited acts, and on no other. The action of the uterus in a parturient women being essentially natural, and consequently healthy in its operation, would not therefore be involved in the action of the remedy, separating, as we must do, the unnatural spasmodic condition of the cervix from the natural uterine contractions; but if the opiate was given when

no spasm existed, then, as already pointed out, it would be likely to expend its force upon the uterine pains, probably through the medium of the medulla oblongata, and cause either a modification of them or a temporary cessation.—*Obstetrical Journal, Sept.* 1877, *p.* 369.

74.—ON EXCESSIVE CAUTERISATION OF THE CERVIX UTERI.

By Dr. J. Wallace, Honorary Assistant Physician to the Ladies' Charity and Lying-in Hospital, Liverpool.

Now-a-days, the speculum vaginæ is used more frequently by specialists and general practitioners than twenty or thirty years ago, and the public have so overcome their objections to it, that there is reason to fear that its use may degenerate into routine, with consequences so injurious that the instrument may be brought into disrepute. This would be a misfortune for the patient, and a crippling of the resources of the practitioner. Nevertheless, if, as a matter of routine, it be passed once or twice a week for months or years, for the purpose of cauterising "an ulcer of the womb," this danger is apt to arise. I know not whether this practice arises from the fact that students are not taught gynæcology, and hence become practitioners with this branch of their art still to learn, or from a want of due consideration of the circumstances which surround this special branch; but whichever it may be, I feel sure that, to obviate all this, the licensing bodies ought to extend the term of study and increase the number of lectures, in order to promote the knowledge of obstetric medicine and surgery.

Excessive and Prolonged Cauterisation.—What are the immediate pathological results of weekly or bi-weekly application of the nitrate of silver, which is the agent in most favour with practitioners, to the os tincæ? Inflammation and ulceration, lit up again and again by each application, followed in some instances by atrophy, and in others by hypertrophy of the cervix, with hard painful cicatrices and more or less contraction of the tissues and closure of the os, or even complete obliteration; endocervicitis and endometritis, the mucous lining of the cervical canal being more or less destroyed, and an ulcerated irregular surface remaining instead. Secondary functional derangements of the uterus follow, manifested first by dysmenorrhœa of a metrorrhagic character; and as the obstruction increases, regurgitation of the menstrual fluid takes place through the Fallopian tubes, setting up ovarian, perimetric, and general pelvic inflammations, with all their subsequent miseries.

The portio vaginalis uteri in some cases is so destroyed by caustics, that a hard depressed cicatrix alone remains of it, and this is generally painful to the touch; the os is small, but not sufficiently so to account for the severe dysmenorrhœa, which I have heard described by intelligent patients as "awful agony which sent them delirious." In such cases, the mucous membrane of the cervical canal is disorganised, and covered with uneven granulations which bleed on the most gentle touch of the sound, and are so acutely sensitive that I have frequently found it necessary to give chloroform before it was possible to examine the condition of the uterus. In a case under my care lately, the canal was tortuous from these excessively sensitive granulations; yet, on passing the speculum, the mucous membrane of the cervix seemed free from all inflammatory irritation, and it was only by a minute examination of the canal of the cervix that it was seen to be denuded of healthy membrane, while a careful digital examination revealed to the touch irregularities marking the different cicatrices. Sexual intercourse could not be tolerated; uterine pain was ever present; and although she had been cauterised externally and internally by various practitioners during a period of several years, no relief was obtained. The cicatrices were clearly seen with the aid of the duck-bill speculum. This uterus had never been impregnated, yet it measured fully two and a half inches, although its vaginal portion had been destroyed to more than half its extent. Relatively, it was enlarged from menstrual obstruction. Free division of the cervix cured this case.

Painful cicatrices are also frequently seen upon a hypertrophied cervix, accompanied by the same pathological degeneration of the mucous lining of the canal as in cases of atrophy, and they may exist where the conditions are apparently normal. Such cases as the latter are the opprobria of gynæcologists. They go the round from one practitioner to another, unrelieved, and ever complaining. I saw some time ago a patient of this sort, who had been almost everywhere, and had suffered everything to be done which had been recommended for her case, yet the uterine aching pain remained as before. The hard and nodulated cervix was painful to the touch, although its mucous covering was smooth. The sound passed with difficulty on account of the tortuous cervical canal; and, although no force was used, free bleeding followed. With the speculum, the os was seen to be small, puckered cicatricial lines radiated from it, and the canal was denuded of its membrane and ulcerated. Although married twelve years, this patient had never been pregnant, and was in other respects healthy. Under various practitioners, she had been submitted to various forms of treatment, but without any abatement of the dysmenorrhœa, metrorrhagia, and leucorrhœa. The canal

of the cervix was freely laid open with the uterotome, and the painful cicatrices removed by excision. She recovered rapidly, and it may be remarked that, as is usual in these cases, the cervix healed in a month without any caustic application whatever; carbolised water and simple astringent injections used daily being all that the case required. Of course, it was necessary to prevent closure of the os during the healing process by the occasional passage of the sound. This was the first case in which I performed this operation for painful cicatrices; and, as I had the opportunity of watching this patient for six years afterwards, it is interesting to state that she had no return of her uterine disease.

These two cases illustrate the methods of treatment by incision of the canal and removal of the painful cicatrices, where uterotomy fails, or where it may be thought not likely to be sufficient, as in hypertrophy, which I have practised for nearly ten years with almost invariable success, and it has been equally successful in the hands of my friend Dr. Steele, who was induced by me to try it. Some years ago, at a meeting of the Lancashire and Cheshire Branch of the British Medical Association, he read a paper upon this subject, in which he detailed the results of several successful cases.

Another unfortunate condition arising from the application of caustics to the cervix uteri occasionally met with is—

Loss of Sexual Desire, which may be partial or complete.—On several occasions, I have been consulted by patients for this condition, in which sexual connection was only tolerated, and where there was no sexual orgasm whatever. The fear was, lest any unhappy family troubles should arise by discovery on the part of the husband. An inquiry into the causation of these cases has invariably elicited a history of frequent cauterisation, followed by loss of special sensation. Having had my attention thus accidentally drawn to this subject, I availed myself of every opportunity of inquiry which presented itself to me; and now, from having observed a number of cases, one of complete and permanent loss of sensation, one of complete loss remaining for two months, after which the normal condition returned, and several others where the loss was more temporary after each caustic application to the uterus, but still so marked as to be noticed by the patient, I am convinced it is associated with peripheral lesion of the nerves of the mucous membrane of the cervix, cicatrices producing permanent compression and loss of function, and probably with changes in the nerve-centres from sympathetic irritation.

Atresia uteri may occur at any stage of the caustic treatment. Three cases have lately been under my observation. The first was complete in a patient between fifty and sixty years of age,

produced by bi-weekly cauterisation for ten months ten years ago, for ulceration during the climacteric period, and again two years ago for eighteen months by another practitioner. She complained of a burning aching in the womb, intense periodic spasms, the pain radiating to the sacrum, around both hips, and down both thighs, accompanied with palpitation, profuse sweating, and not unfrequently delirium. On vaginal examination, a depressed hard cicatrix occupied the place of the cervix. With the speculum no os could be found. Having brought down the uterus with the velsellum, I passed by force a small steel probe through a depression of mucous membrane which seemed to indicate the former situation of the os, into the canal of the cervix, and speedily opened up the canal to its natural size by a succession of bougies. The uterus measured two and a half inches, and contained a small quantity of mucus, not amounting to hydrometra, but sufficient to produce the uterine disturbance. It may be noticed that the uterus was hypertrophied, considerable atrophy generally occurring at her age. The atresia of age takes place at the internal and not at the external os, and in that it resembles the adhesive form of atresia which sometimes follows metritis after confinement. The next case is that of a young married patient, the vaginal portion of whose uterus was nearly in the same condition as that of the last case. No os could be discovered by touch or sight, and the cervix was represented by a hard round nodule of the size of a large pea, but the uterus was small. During menstruation, the discharge was discovered oozing from a pin point opening at one side of the cervix. With difficulty, a steel probe was passed, followed by a larger and larger one, and immediate relief followed a profuse discharge of retained menstrual fluid. Ten days afterwards, I opened up the canal with the uterotome, and she is now well, metrorrhagia and dysmenorrhœa having disappeared, while the general health is restored This patient had been under the care of a practitioner for eighteen months, who passed the speculum twice a week and applied caustics. And for what? She had been married two years, and not becoming fruitful, yet wishful to, had consulted him, and he had "found ulceration." But this could hardly have been the ulceration of a normal uterus, for it was undeveloped and only measured an inch and a half in depth. Another case of incomplete atresia, accompanied with considerable hypertrophy of the cervix, I saw a year ago. She had become disheartened and hopeless of cure, having derived no benefit from treatment; and had it not been that I was summoned to relieve her intense agony produced by obstructed menstruation, the case would not have come under my notice. The cervix was irregular and nodulated, measured an inch and

a half in length, and although the os could be discovered, the smallest probe could not be passed, and the touch requisite to do so could not be tolerated. Narcotics relieve her, and as she has been told that when the menopause comes her suffering will cease, she declines to submit to any further treatment. Cauterisation weekly for two years preceded the morbid condition.

Acute Atresia Uteri, if I may so call it, is another form of the lesion produced in a more direct and speedy manner by the recent and more active form of treatment of swabbing out the cavity of the uterus with nitric acid, acid nitrate of mercury, potassa fusa, to the cervix, etc., for endometritis and inflammation of the cervix. The cases I have seen were partial, and occurred in my own practice. Four applications of acid nitrate of mercury, extending over a period of four months, produced the first case. Her endometritis was cured, and I did not see her again for several months, when she returned complaining of painful and obstructed menstruation. The os admitted a small probe; the cervix was hard, and any attempt at dilatation was intolerable. Chloroform was administered, and the canal laid open with a successful result. The second case was brought on by nitric acid, but was discovered at the end of two months, and yielded readily to dilatation. The third and fourth cases resulted from potassa fusa applied to the cervix, and were discovered at the end of three months. Immediate dilatation and the occasional passage of the sound set them right. In producing rapid dilatation in these cases, and in keeping the os open in cases where there is a strong tendency to contraction and closure, I have found the greatest assistance from Priestley's dilator.

In conclusion, I may remark that to Dr. Henry Bennet the profession are chiefly indebted in this country for the many valuable uses, and perhaps, through misapprehension on the part of practitioners who endeavour to imitate his practice, for many of the abuses of the nitrate of silver. He says, (On Inflammation of the Uterus, p. 393) : "The solid nitrate of silver, or a strong solution, should be applied every three, four, or five days to the inflamed mucous membrane covering the cervix." This treatment is recommended for many weeks until the ulceration heals. Again, he states (*ibid.*, p. 417) : "Sometimes, indeed, the os becomes smaller than in the healthy state, but I have never seen it obliterated by these severe cauterisations,' viz., by potassa fusa, &c., "for the natural secretions of the regions above always gradually re-open the os, &c." West ridicules somewhat and opposes the treatment of Bennet. Bernutz (Diseases of Women, translated by Dr. Meadows, p. 140) says : "I cannot too strongly protest against the intemperate use of caustics to the cervix uteri," and mentions one case where "obliteration of the cervix

resulted;" and in another "contraction of the cervico-vaginal orifice;" and in a third, of the "cervico-uterine orifice." These cases, as shown by him, lead on to more serious consequences. Yet in the Olistetrical Journal, February 1877, a case is given from the "Archives de Tocologie," (September 1876), reported by a countryman of his own, Dr. Chambaud, where for uterine hemorrhage at the third month of pregnancy, after leeching, and because of a muco-purulent discharge from the os, cauterisation with nitrate of silver, by means of Lallemand's *porte-caustique*, of the os and cervix was continued for fourteen weeks. Complete occlusion followed, and although seen in consultation by M. Depaul, it could not be overcome either by the finger or sound. When labour came on, fourteen hours of pains recurring every two minutes made no impression, and it was only after incision that the uterus was emptied. The child died, and the mother ultimately recovered after an attack of metritis. Wonderful to relate, Dr. Chambaud thinks that had he cauterised more persistently than he did, no occlusion would have taken place. Thomas (Diseases of Women, p. 587), affirms that strong caustics are a prolific cause of contraction of the cervical canal, and Rigby (On Female Diseases, p. 114) speaks of the "dishonest application of caustic to the os uteri at intervals so short as to render it impossible for the effects of the first application to have healed before the second was made."

Here I may state that repeated cauterisation is not necessary to produce healing, for the severe ulceration produced by potassa fusa on the lips of the uterus will heal in from four to six weeks without any subsequent cauterisation whatever. It is only necessary to direct the patient to use the vaginal douche daily, either warm water, carbolised water, sulpho-carbolate of zinc, or whatever astringent in solution that may be deemed suitable. The occasional passage of the sound during the healing process will obviate any tendency to atresia.

The treatment may be summarised thus. 1. For painful cicatrices, divide the cervix freely; if that fail, remove them by excision. 2. For recent atresia, use rapid dilatation; for atresia of old standing, free division with the scissors and uterotome; simple medicated dressing afterwards, if there be endometritis. 3. The sound should be passed a week before each menstrual period for three or four months to prevent reclosure.—*British Medical Journal, Oct.* 6, 1877, *p.* 472.

75.—ON QUININE AS AN ECBOLIC.

By Dr. John Paterson, Surgeon-Superintendent "British Seamen's Hospital," Constantinople.

A case occurring in my practice here seems to confirm the statement of Dr. H. P, Roberts, of Baroda, as to the ecbolic action of quinine, and may be worth recording.

The patient, age twenty-four years, in good health, was near the eighth month of her first pregnancy. On the 16th March she took a haphazard dose of quinine, supposed to be about ten grains, for an imaginary threatening of fever (ague), of which she had had two or three attacks before marriage. In less than an hour after most active labour pains came on, and before I could arrive, she was delivered of a living male child, strong and healthy for his age. The membranes immediately followed. Strong uterine contractions continued for two hours, but in every other respect, she did well.

Careful inquiry on all points relating to her health and condition leads to the conclusion that the quinine alone induced such sharp uterine action, and that it was not a feverish attack coinciding with the administration of the drug. Even admitting this to some extent, there would appear to be need of caution in the administration of quinine to pregnant women, as most practitioners in countries where ague prevails frequently observe that a large dose does, at times, produce an unusual degree of nervous irritability and uterine disturbance.

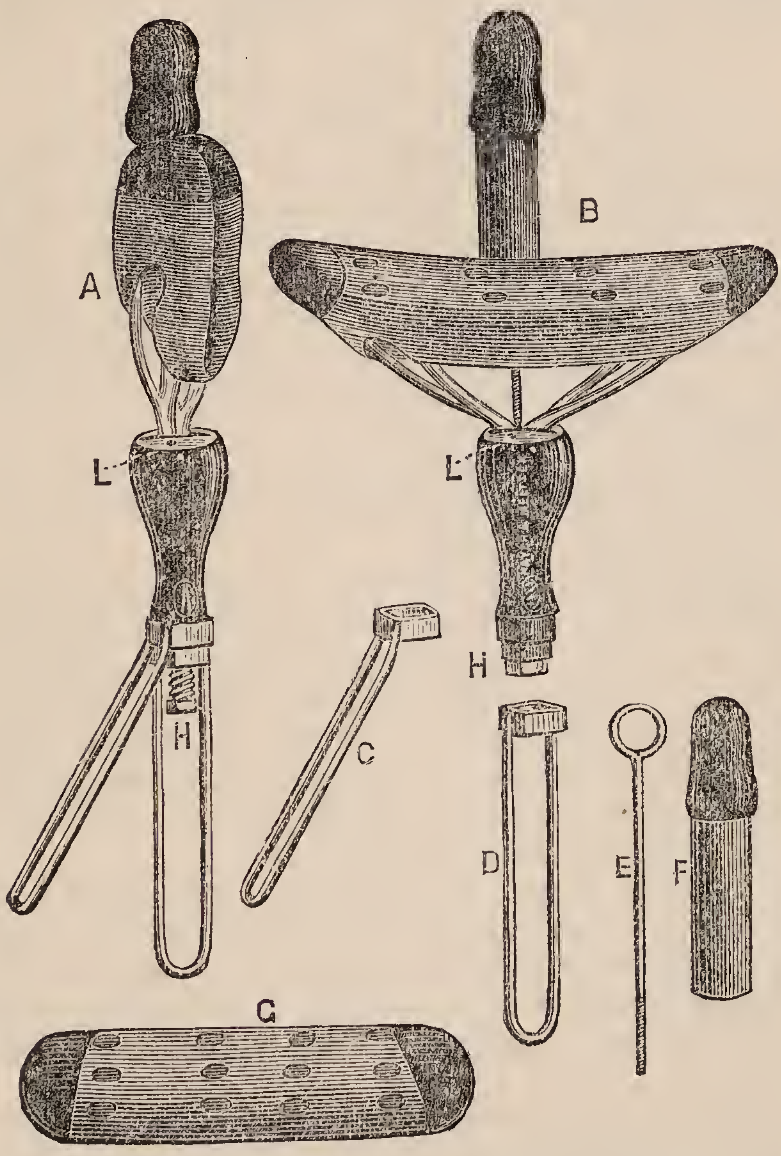
The popular notion that attributes to quinine this action is not confined to the natives in India, it is general all over the East.—*Practitioner, July* 1877, *p.* 36.

76.—COMBINATION PESSARY.

By Dr. Richard Neale, London.

The accompanying woodcut represents a pessary I have found exceedingly useful in several obstinate cases of uterine displacement, where all other appliances have proved less effectual. It will be seen that it is a modification of "Zwank's," but made so short in the stem that when inserted by the aid of two handles (D, C) it remains wholly within the vagina. An elastic perforated cap (G) supports the prolapsed uterus. If there be retro-version or flexion, a malleable silver wire (E), supporting an india-rubber cap (F) is screwed into the stem posteriorly. This cap, by being raised or lowered, regulates the height of the upright, or the wire itself can be bent or cut to meet this need, if too long. Anteriorly (L) a similar arrangement meets the difficulty of ante-version or flexion. The support may be varied in form if desired, being more of a T or crutch shape. Frequently the large cap is not needed, and the anterior or posterior upright is all that is required. The nut (H), by being screwed up or down, regulates the extent to which the blades can be expanded; this is, perhaps, scarcely needed, and in later instruments I have not had this affixed. To insert, the patient lies on her back with her knees drawn up. Having closed the instrument with handles fixed (A), and seeing the supports are

in correct position, she, if for retroflexion, passes the upright close against the anterior rectal wall as high as possible; then screwing the key (D) from left to right, the blades open, and let the os uteri fall between; still pressing gently upwards, and steadying with the lateral handle (C), the key is turned as far as needed, bearing backwards at the same time, so as to lever up the fallen body or fundus uteri. If the case be one of anteversion, the support, being fixed in front at (L), is carefully carried in front of the os, and as the instrument is opened, the handle is carried forwards, so as to make the upright act as a lever in the contrary direction to that required for retroflexion.



If the pessary be inserted with its curve looking backwards there will be no friction under the pubic arch.

Those who have tried it are greatly pleased with the facility of insertion and extraction; each night the instrument being placed in Condy's fluid, ready for morning use.

The chief points of the "Combination Pessary" are: 1st. Self-use. 2nd. The os is not bruised by impinging on any hard surface, as in ordinary Zwanks. 3rd. No pessary for self-introduction has, as far as I know, yet met the need of such cases as those of retro- and ante-version, &c., although, doubtless, some one will find out that Shakespeare knew all about it, and had described it fully years ago. 4th. The facility with which the same pessary may be used for the various displacements of the womb.

I owe much to the perseverance and skill of Messrs. Mayer and Meltzer in carrying out and improving upon my original idea.—*Lancet, Aug.* 18, 1877, *p.* 262.

77.—COTTON-WOOL TAMPONS.

By Dr. Henry Lippert, Nice, France.

It has occurred to me during my recent visit to the Gynæcological department of the London hospitals, that in the treatment of uterine displacements, congestions, and vaginal catarrhs, the use of cotton-wool tampons is not so general as, in my opinion, it deserves to be. Intra-uterine stems, pessaries in all their various forms, &c., often prove irritating, and, instead of subduing, frequently increase the congestion of the surrounding mucous membrane.

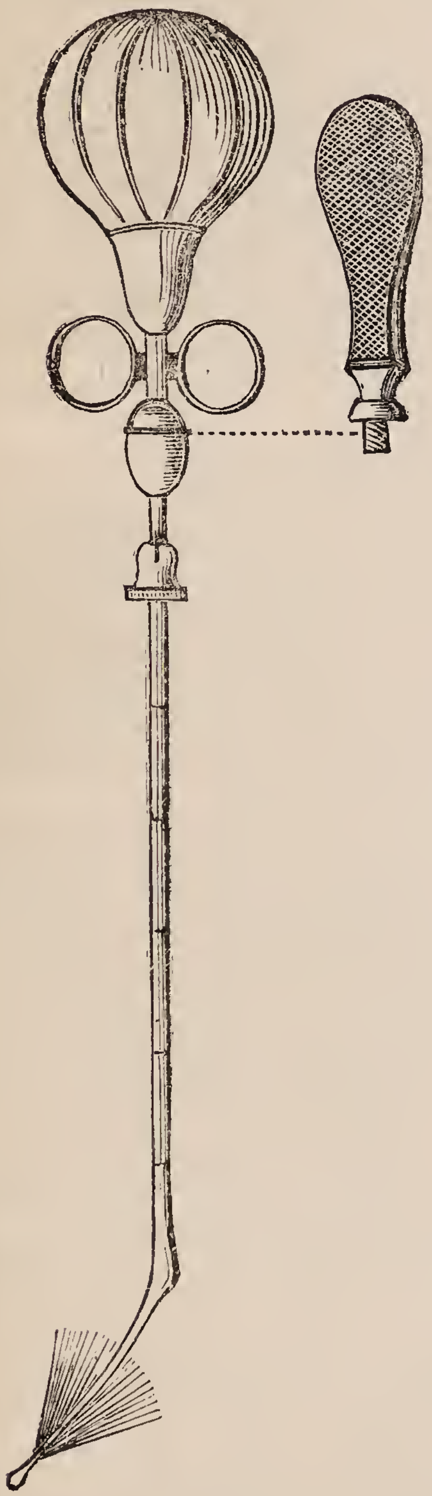
Injections, notwithstanding their beneficial effect, are mostly insufficient through the shortness of contact of the injected liquid with the diseased parts.

The regular use of cotton-wool tampons, on the contrary, secures a more effective treatment through the following two advantages:—1. The tampon forms a support sufficient to keep up the womb without exercising any undue pressure on the vagina, os uteri, bladder, or colon. 2. When dipped in an astringent solution, such as chlorate of potash, alum, tannin, perchloride of iron, diluted acetic acid, &c., it has a continued contracting effect on the relaxed parts.

It has occurred to me that there is a want of a cheap and practical tampon-holding instrument, by which every woman, whatever be the amount of her nervous irritability, or however unskilled her hands, would be able to introduce, without the slightest difficulty, a tampon to the laquear vaginæ, touching the os uteri.

An instrument is manufactured in France, after my design, in olive-wood, which I have employed for years in my practice, and which, I believe, fulfils the above conditions. Believing it might prove useful to my English colleagues, I have given the model during my stay in Manchester, where I assisted at the meetings of the British Medical Association, to Messrs. Wood and Co., surgical instrument makers, King-street, Manchester. —*Lancet, Sept.* 1, 1877, *p.* 336.

78.—NEW COMBINED SOUND AND SYRINGE, FOR UTERINE PURPOSES.



A combination of uterine sound and syringe is a desideratum which has been found in use most convenient. The instrument can be used as an ordinary uterine sound, but when, as in many cases, it is desirable to introduce fluids into the cavity of the uterus, the combination will be found extremely convenient, inasmuch as the douche can be effected without the necessity of removing the sound. The sound is hollow and has a bulbous extremity to facilitate introduction; it is perforated diagonally, so that the fluid flows in the reverse direction, and douches the entire extent of the uterine cavity. It is made by Messrs. Salt and Son, Surgical Instrument Makers, of Birmingham. — *Lancet*, 1877.

79.—SPECULA MADE OF GLASS.

By JAMES MURPHY, Esq., M.B., Sunderland.

Among the gynæcologists of the present day the cylindrical speculum has passed almost completely from the department of diagnosis into that of therapeutics; but, as a therapeutic agent,

it still holds a foremost place as an instrument by means of which we can most effectually apply local remedies for the treatment of the various diseases of the vagina, the os, and cervix uteri. I wish, therefore, to draw the attention of the profession to certain advantages which specula, made of glass alone, possess over those made of other materials. I am the more anxious to do this, as in none of the text-books with which I am acquainted are they recommended, though I feel confident they only require to be tried to become generally used.

The first advantage they possess is cleanliness, for there is no ridge as is usual at the inner end of metallic, or crevice which often forms at the end of coated, specula after a little use, where syphilitic or other virus may lodge. A source of danger which it is needless to dilate upon.

Glass specula are easily washed, and, as they are transparent, one may see at a glance if they are perfectly clean; and this I consider a most important property, as by it the slightest particle of foreign matter may at once be detected. Secondly, the walls of the vagina may be seen through the glass, and fistulæ or other morbid conditions recognised; and in anteversion of the uterus, the cervix can easily be discovered and readily brought into the lumen of the speculum. And, finally, they give a very good light, are not acted upon by medicinal agents, and cost much less than the specula now in use.

The shorter they are made the better, provided they answer the purpose. The glass should be pretty thick, and smoothly bevelled at each end. The diameter should be the same throughout—no tapering, except the usual funnel-shaped expansion at the outer end. The inner end should be cut obliquely in some, as easier to manipulate a displaced cervix with; and straight in others, for use when it is desired to produce an ectropion of the lips of the os, so as to gain a view of the cervical canal.

I have been much pleased with some that Messrs. J. Wood and Co., surgical instrument makers, York, have made for me, but I hope soon to be able to get them made up of De la Basties's toughened glass, which will render them less liable to be broken.—*British Medical Journal, May* 26, 1877, *p.* 644.

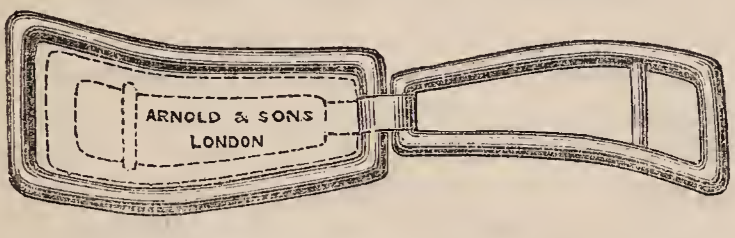
80.—A NEW UTERINE REPOSITOR AND RETRACTING SPECULUM.

By Robert T. Cooper, Esq., M.D., Univ. Dubl.

[The instrument described in this paper is considered by its inventor a handy and efficient instrument for rectifying malpositions of the womb. It is constructed of metal and made by Messrs. Arnold and Sons, London.]

It will be seen that it consists of two arms, joined by a hinge, such that, by the aid of a stop upon it, extension is

limited, so as to leave the arms forming a long, straightish instrument when opened out in one direction, while, in the other direction, rotation is confined to allowing the arms to form with each other an angle of about 55 degrees. In the former case the instrument serves in malpositions of the womb as an eminently useful repositor; in the latter, that in which the blades open at an angle of 55°, it forms a retracting speculum, upon the same principle as Sims' duck-bill.



As a repositor the mode of using it is this—for antiversion, the patient being placed in the usual obstetric position, the smaller arm of the repositor passes up underneath the pubic arch lying between it and the operator's right hand, his index finger acting as a guide to the extremity of the instrument, by pressing against the cross-bar placed about half an inch short of the end, and so impelling it onwards; in this way the smaller arm of the instrument is under the operator's entire control, the larger arm, owing to the hinge-connection, preventing the possibility of its becoming unmanageable. The simple advantage of this repositor, therefore, is that it gives additional length by half an inch to the operator's finger, and its end being wide, as well as non-resisting, it allows of the application of a steadier force to a larger surface than could possibly be obtained by the unaided finger. The *amount* of force required to effect the return of a misplaced womb is, I may observe, in the great majority of cases, comparatively trivial, very slight pressure in the required direction being all that is necessary to attain our object. This arm of our instrument being gently curved an inch from the end, will allow of our exerting pressure against the fundus of the womb upwards and backwards—that is, in an arched direction along the antero-posterior axis of the pelvis. These particulars being explained, its utility in cases of retroversion will be apparent—the only difference here being that we will find it an advantage to direct our patient to lie on her back, and allow the instrument to rest against the floor of the vagina, the operator's hand with its index finger passing up in front of the instrument. Thus we pass it in, resting it between the hand and pubic arch for antiversion, upon the vaginal floor for retroversion, and in either case it proves, in the great majority of instances, an equally efficient instrument for the required purpose.

The larger arm of the repositor will be found useful in aiding manipulation when returning a prolapsed womb, and by the aid of a movable hinge-pin, we can render the instrument additionally serviceable in facilitating the return of an inverted womb, both of the blades being then employed in conjunction with the operator's hands.

I have had this instrument constructed with an additional cross-bar, an inch and a half behind that described, but which is not represented in the plate. This enables us to use the repositor for the purpose of a speculum, upon the same principle as we do Marion Sims' duck-bill speculum—namely, for exerting retraction against the vaginal wall, and so to allow of air, or, as Sims will have it, (*vide* M. Sims' "Clinical Notes on Uterine Surgery"), the atmosphere, to enter the vagina, and thus induce an expansion of its walls. The patient, for this purpose, should, as Sims points out, be placed in the genu-pectoral or in a semi-genu-pectoral posture—that is, with her buttocks partially lifted up by the aid of some such prop as a couple of pillows placed underneath would afford. In some cases our retracting speculum possesses practical advantages over Sims'—thus, in operating for recto-vaginal fistula, it helps to steady the sides of the wound while the needles are being inserted; and, in cases of abscess of the walls of the vagina, it affords a ready means of exerting pressure against the walls of the abscess, while space is obtained for the discharge of its contents through the blade of the speculum; and, lastly, our instrument is extremely convenient, inasmuch as it is so portable.—*Dublin Journal of Medical Science, May*, 1877, *p.* 457.

81.—FÆCAL ACCUMULATIONS, SIMULATING UTERO-OVARIAN TUMOURS.

By Dr. G. de Gorrequer Griffith, Senior Physician to the Hospital for Diseases of Women and Children, &c.

In the early days of my practice I had a female child brought to me with an enlargement in the left iliac fossa. I pronounced it to be a tumour, from its slow growth, its hard nodular condition, its only slight mobility, and from the entire history. I could not satisfy myself as to its nature, and asked one of my seniors in the profession to see the girl along with me. He thought also that it was a growing tumour; but he would not commit himself to a determinate diagnosis at this, his first visit, and we agreed to watch the case together. The bowels, we had been assured, had been freely moved every day. Before our next visit the child had been sent away; and I learned that the enlargement had soon been reduced by means of aperients and enemata.

From that case I learned a valuable lesson, which has not been lost on subsequent patients.

The second case which I shall here quote is as follows:—In the autumn of 1876, I was hurriedly summoned to an old lady, who had, within a few days of my seeing her, met with a severe accident in the city, having been knocked down by a hansom as she was crossing the street. All her friends had given her up to die. She was so powerless to move, so prostrated, and so large a tumour, they stated to me, had made its appearance since her injuries. Her age (80) seemed to exclude all hope of recovery; and I was asked to see her—more that it should not be said she had died incapable of making her will, and to witness her signature to it, than with any idea that I could benefit her.

I examined the abdomen, and while doing so, learned from her that she thought she had been larger on the left side for some time before the accident. I found considerable enlargement of the entire abdomen from flatulent distension, and on the left side a tumour, hard and apparently irregular, extending from the left hypochondriac into the left iliac fossa, and passing a little way to the right of the mesian line. At first I thought it might be enlarged spleen, or a left ovarian dropsy, or an extra-uterine fibroid, which had been unnoticed, and was now observed, solely because attention was directed to the left side, where the patient had been struck by the vehicle. I could not at this, my first visit, make a very minute examination, owing to the extreme prostration and depression; but at my second visit, having in the interval built her up and cheered her all I could, I examined very carefully per vaginam, and with equal care explored by the rectum. I then came to the conclusion that there were neither ovarian nor uterine tumours, and that I had to deal with an accumulation of fæces—even though the bowels were moved every day, as the attendant informed me, and that the accumulation had commenced previous to her accident, forming no doubt the enlargement which she told me she had noticed before her injury, and which, as the accumulation increased, culminated in the enlargement I found. I swept out the bowels by free purgation, kept up for some days, while I sustained her with light and easily digested nutrients, allowing as stimulant only good tea and coffee.

And here, in parenthesis, permit me to say, that in all ailments, when I can get a patient to follow my advice, I prefer as stimulants—non-medicinal—tea and coffee *made with boiling milk*, instead of with water, a plan I invariably follow also with infants and children with most pronounced good effect.

To return to our case. The purging quite dissipated all

enlargements, and the old lady's will has not yet made its way to Doctor's Commons. In this case, though I should have wished it, I was precluded from having injections given, because the patient objected, preferring to try the effect of aperients by the mouth; moreover, she would not have a trained nurse, and being a private patient, I could not compel her. Had the medicines given by the mouth been inoperative, then we should have tried coercion in the matter of the enemata; but as the aperients acted speedily, efficaciously, and in every way that could be desired, producing neither emesis, nausea, nor any other unpleasant effect, I followed the patient's inclination, and have no cause of regret as to the results.

The next case which I shall quote is that of Mrs. G., aged 25, mother of three children, the last being about four months old when I was first in attendance. I was called up to her on the night of 18th June 1876, "as she was suffering acute pain in the left side, which she could endure no longer." On examining the abdomen I found a hard, irregular, exceedingly tender tumour, from which she was enduring great agony, and which was almost as large as an infant's head. I made no further examination that night, contenting myself with ordering her ½-grain morphia suppositories, to relieve not only the pain, but likewise the tenesmus and the passing of mucus. The discharge from the bowels was quite fluid, but distinctly fæcal, occasionally a scybalous mass making its appearance.

Next day, the morphia having taken good effect, I examined with the finger by the vagina, but could make out neither ovarian nor uterine tumour; the sound *in utero* enabled me to make certain that there was no intra-uterine growth; but movement of the uterus with the sound in the interior of it was attended with movement of the mass, which I found lay outside the womb, yet connected to the left and upper portion of it—in fact, attached to it. I gave it as my opinion that whatever the mass was, it was outside the uterus, and was adherent to it, and that it was not ovarian. I did not, however, express the opinion at which I arrived after the above examinations and after thoroughly exploring by the rectum, viz., that it was a case of impacted and accumulated fæces, which, having set up great irritation, had occasioned inflammation, effusion of lymph, and matting or glueing of the bowel to the left and upper portion or cornu of the uterus, that organ being still enlarged, its involution after delivery being not yet completed, probably owing to the irritation, inflammation, and subsequent adhesion to which I have referred. Taking this view of the case, I purged freely and continuously for some days, till at length after the lapse of six weeks I had the satisfaction of hearing from my patient—for I did not attend her

continuously during this period—that the tumour was all gone, and she was quite well; facts I verified by careful manipulation when she last visited me. The iodide of potassium had been combined with the aperients, as had also anodynes—the former in hope of dissolving adhesions, the latter with a view to ease pain. I would add, to show the difficulties which sometimes behedge the diagnosis in these cases, that this patient had previously had pronounced to her by three medical men that operation alone (gastrotomy) could do her any good; and of this she had a mortal dread, so that all through I buoyed her up with the hope that the knife might never be required.

The swelling had commenced to be noticed about twelve or fourteen days after the birth of her child, was chiefly confined to the left side, though sometimes it seemed to enlarge, and to extend higher up and across the middle line towards the right, and was so large that it was as though she was at her full time, and when walking, even across her room, she required a towel to support the abdomen; at other times it would subside, preserving, however, the same shape; these alterations in size were synchronous with the action of the bowels, and gave me a valuable clue. The agony had been very great, and she told me nothing had relieved her for any length of time till she had used the morphia suppositories. At no period was there a discharge of matter indicative of any internal abscess; nor any flux of water either into the abdominal cavity, or into the bladder, or any way externally, which would demonstrate the existence and rupture of an ovarian or other cystic growth; therefore, the only diagnosis at which I could arrive was that the bowels had become blocked during the confinement period, had not emptied themselves fully, that an accumulation occurred and became greater and greater, being however occasionally partially lessened by the aperient action of the bowels themselves, which accounted for the diminution and subsidence that had been noticed in the swelling.—*Edinburgh Medical Journal, May* 1877, *p.* 991.

82.—TREATMENT OF POST-PARTUM HEMORRHAGE.

By Dr. H. Otis Hyatt, Kinston, North Carolina. U.S.

[Practitioners who have no surgical instrument maker within 300 miles are naturally thrown a good deal on their own resources, whatever these may be. Dr. Hyatt under these circumstances has frequently found "the Condom" a friend in need. In an article upon "Fifteen uses of the Condom" the eighth use is "as a means of arresting post-partum hemorrhage."]

For this purpose the Condom—or what is better, as it admits of very great distension, an india-rubber balloon, which may

be bought at any toy-shop—is tied over the end of a Davidson syringe nozzle, and passed into the cavity of the flaccid uterus. It is then distended by warm or cold water; by this means we bring pressure to bear directly upon the mouths of the bleeding vessels, which effectually seals them, and renders further hemorrhage impossible. It might be objected to this method, that after the uterus had been distended it might refuse to contract. But this seems hardly probable, and, even should such an event occur, we can, after the woman has somewhat recovered, administer ergot in sufficient doses to produce contractions. As soon as the distension required to check the hemorrhage is reached, we pull the tubing from the syringe bulb and compress it (the tubing) between our thumb and forefinger; by so doing we have perfect control of the water in the bag, and can allow it to escape *pari-passu* with the uterine contractions.

Since the above was written I have lost no opportunity for testing the merit of this method of arresting post-partum hemorrhage. In fact, I have used elastic pressure, either by means of a Condom, rubber balloon, or a Barnes's bag in all the cases of post-partum flooding that I have since attended, and have been so well pleased with the result that in July, 1876, I sent a short paper to the American Journal of Obstetrics, which was published in October of the same year. From this paper the following is a quotation:—" The procedure is simple, safe, and effective, and up to now has never failed in a single instance in which I have used it. It consists in passing into the cavity of the uterus a rubber bag, which is afterwards distended by means of air or water, preferably water, until it fills the entire uterine cavity. The bag will press in the direction of the least resistance, and adapt itself to the little inequalities of the placental site. We thus, without using any great amount of force, bring sufficient elastic pressure to bear upon the mouths of the bleeding vessels to effectually seal them, and render further hemorrhage an impossibility.

"The hemorrhage being arrested, we can leisurely direct our attention to inducing uterine contractions by kneading the uterus and the administration of ergot, either by the mouth or hypodermic injection. When uterine action sets in (and it occasionally will very soon, especially if ice water be used to distend the bag with), we can allow the air or water to escape *pari-passu* with the uterine contractions, or, better, allow it to remain. The bag being elastic, is easily moulded to the parts through which it has to pass, and will be expelled just as the bag of waters would, and at the same time continue to act as a valve to the bleeding vessels until the womb is so thoroughly contracted that further hemorrhage will cease."

I then give the details of three illustrative cases, and conclude by using the following language:—"This method has several advantages over those generally used for the arrest of post-partum hemorrhage. Even leaving out the elastic pressure, it is better than passing ice into the uterine cavity, should we be disposed to rely upon cold as a means of astringing the bloodvessels, and inducing uterine contractions. We can inject the bag with ice-cold water, and save the woman the annoyance of a cold bath to the buttocks, which will surely take place as the ice in the uterus melts, and the water runs from the vagina. Its advantage over pressing the placental site with the hand is considerable; first, it is perfectly reliable, the distended bag will cover the entire site, which the hand will not always do; and besides there is not the least danger of bruising the uterus as there is when one hand is placed upon the placental site, and is pressed upon by the other hand over the abdominal wall. Over the persulphate of iron injections its greatest advantage is its perfect harmlessness; we run no risk of inducing metritis or puerperal septicæmia, and besides we avoid the disagreeable stickiness of the hands, which one is sure to have if they are brought in contact with the iron injection. But its chiefest and greatest advantage over all other methods is, that it can be more speedily resorted to, and does its work more quickly. One or two minutes is generally long enough to fully distend the bag, and check the flow. It is perfectly harmless, and the most awkward need not hesitate to resort to it. Even should the bag burst, which is the only accident that can possibly happen, we simply wash out the uterus, which cannot possibly do harm; that is, if the bag had been filled with water, which is, I think, best and safest, though I have frequently used air, and had no accident."

During the same month (October, 1876) that my paper appeared in the American Journal of Obstetrics, there appeared in the Obstetrical Journal of Great Britain and Ireland a description of Dr. Chassagny's method of inducing premature labour, which was published in the Archives de Tocologie for May, 1876, from which the following is a quotation:—"But it is in post-partum hemorrhage that the results are more striking. The uterus is first emptied of all clots, and the bags are then introduced and dilated with water. The thin bag then insinuates itself into the uterus and completely fills it, compressing the open mouths of all the bleeding vessels. When the uterus commences to contract it may be allowed to expel the water, the open mouth of the tube being kept at a high level. There is thus perfect security that no cavity is formed into which hemorrhage could take place. The author has saved by this method two patients who would otherwise inevitably have perished, all other means having been used in vain."

I have made these quotations, first, to show that I had not only resorted to elastic pressure to check post-partum hemorrhage before Dr. Chassagny had, but that my method had been published at least a year and a half before his. In the discussion of Dr. Milne's paper, those methods that were most objected to were plugging and injections. This method which I have proposed will do away with the necessity of ever resorting to either. I think this is the very best and most reliable of all our means for checking post-partum hemorrhage. It is true that ergot stands at the head of the list of hæmostatic agents, but it takes some time for it to act, whether introduced by the stomach or hypodermically, and during this time our patient may flow to death.

I have lately had an opportunity of subjecting this method to the most severe trial possible. I was called to see a lady who had just miscarried of a three and a half months' embryo. The placenta was attached high up, with probably a small part detached. She was bleeding freely, and was very weak and nervous. I attempted to force my hand into the uterus to remove the placenta. This gave her so much pain that she begged me to desist, and check the hemorrhage as I had done on a former occasion. (This lady is the first on whom I ever tried this method, which was four years previous to this time.) I passed a Barnes's bag into the uterus, and distended it with water, gave her a large dose of ergot and morphine, and went to bed. Next morning strong expulsive pains set in, and I had the satisfaction of seeing both placenta and bag thrown off before I left. She lost no blood after the bag was introduced. This is the first time I ever resorted to this method before the uterus had been emptied of clots and placenta; but should ever such another case present itself, I will resort to the same means, being fully assured that the result will be all that can be desired.—*Obstetrical Journal, Sept.* 1877, *p.* 383.

[We still maintain that our own method of stopping hemorrhage by the plug is the best (see "*Retrospect*," vol. 70, p. lxii.):—" Clear out the placenta and all clots, run the fingers up the outside of the uterus so as to grasp the fundus, pushing all folds of intestine upwards for fear of injuring them. Pull the fundus downwards whether it will contract or not, now plug the vagina *right well*, so that no blood can possibly escape. What is the result? You have the fundus uteri well down, you have stopped up the opening below,—you cannot have a coagulum much bigger than two fists. This very coagulum acts as a plug in the womb; the *bleeding vessels cannot bleed through a coagulum.* You have thus effectually plugged the bleeding vessels and can wait; but you must keep your hand,

or rather fingers, well grasping the fundus, else the womb may dilate again. In a little time, one, two, or more hours, remove the plug from the vagina, press out the coagulum from the womb, and see if hemorrhage begin again, always giving, as soon as possible, a large dose of ergot. We think the ergot is given in far too small doses; we generally give a half-ounce bottleful of the powder well infused, or two drachms of the liquid extract. We consider all the prejudices against the plug perfectly unworthy of notice. Of course if the hemorrhage return, we plug a second time."—Eds.]

83.—ON SUBCUTANEOUS INJECTION OF ERGOTINE IN UTERINE HEMORRHAGE.

By S. Grose, Esq., F.R.C.S., Melksham, Wilts.

Amongst the means enumerated in Barnes's Obstetric Operations for the arrest of post-partum hemorrhage is ergot, and it is disposed of as untrustworthy. Without doubt such is generally the result in the worst cases of flooding when given in the usual way; and for the simple reason that the stomach is, like all the rest of the body, in a state of intense depression, and quite unable to absorb the remedy. Because the case is desperate, therefore the drug is inert; probably the less dangerous the case the more effective the styptic.

Believing ergot to be a most powerful hæmostatic, perhaps our most trustworthy, owing to its power of causing contraction of unstriped muscular fibre and of the capillaries generally, I have found by experience that Bonjean's ergotine is the most certainly reliable preparation of that drug when administered hypodermically. And when in such a standard work as that referred to above this mode of using it is not mentioned, it may be worth while to again bring before the profession the best mode of its administration; and one point in its favour is, that if it proves inactive, the perchloride of iron injection can still be resorted to. As this latter is admittedly dangerous, it is as well to first try other means which, if inefficacious, are harmless.

Of all classes of medicines, I suppose the hæmostatics give most frequent disappointment to practitioners. And yet hemorrhage from almost any organ adds so gravely to the apprehensions of both patient and medical man that one would have hoped to find these medicines most handy and effective. But who has not been many times disappointed with gallic acid and lead salts, *et hoc genus omne*, in hæmoptysis, in typhoid, in hæmatemesis? In post-partum hemorrhage, all such are considered so useless as to be practically abandoned; and yet these are the very cases where prompt and certain hæmostatic action is "a consummation to be devoutly desired."

On Christmas day, 1875, I was called to a lady in her fourth confinement, who was the subject of a moderate amount of transverse pelvic contraction, which always necessitated the use of the forceps, and who, as labour progressed, required to be chloroformed to restrain her state of frenzied excitement. Her second child had been stillborn. I gave two doses of chloral, which kept her fairly quiet till the pains became severe, and then chloroform as sparingly as possible, by Skinner's inhaler, when pains recurred. The labour was short, and a living child delivered with Barnes's long forceps. Then chloroform was omitted. In about five minutes the placenta came away entire; the binder was removed, and another preparing; she was gradually "coming to," and I felt the uterus through the abdomen well contracted. A few minutes elapsed, when I heard the terrifying sound of a large gush of blood on the floor. She was immediately put on her back, and I compressed the uterus firmly, squeezing out another enormous quantity of blood and clots. She had not quite recovered from the chloroform, was deadly pale, frothing at the lips, and hardly breathing. Getting a relative who was in attendance to keep up firm pressure on the womb by squeezing it in the hollows of her hands placed side by side, and the nurse to remove all pillows, put smelling salts occasionally under the nostrils, and flick the face with the corner of a towel dipped in cold water. I bared the patient's arm and injected subcutaneously fifteen minims of the undermentioned solution of Bonjean's ergotine, which, with a hypodermic syringe, is part of the furniture of my obstetric bag. In case the flooding should continue I prepared a perchloride of iron solution for injection into the uterus, and waited. Within five minutes the womb had firmly contracted, and danger had passed. She had milk given in teaspoonfuls when she had sufficiently recovered to swallow, and very small doses of brandy. Indeed, to my practice of allowing very little brandy in such cases I attribute the slight hemorrhagic fever that followed, and the quick and satisfactory recovery. Here ergot could only have been used hypodermically, and I see no reason to doubt its entire efficacy.

In the chronic hemorrhage frequently so troublesome after miscarriages I find ergotine, subcutaneously given, equally valuable. Lately I saw a poor woman who had miscarried with twins at seven months, and had been "losing" for between twenty and thirty days. She looked as bloodless as if no blood remained in her body. Astringents had been given lavishly, and with next to no benefit. Three days running I subcutaneously injected ten minims of my solution of ergotine, and the hemorrhage ceased. Some time previously, in a somewhat similar case, the solution of the extract of ergot had

proved effective, but only after taking the nauseous doses for some days.

The disadvantage of ergotine so given is the hard dark lump sometimes caused, which teases by itching, and is slow to disappear; but I have never seen it suppurate.

The solution I use is made by dissolving twenty-four grains of Bonjean's ergotine in one drachm each of rose-water and glycerine, and five minims of this equals two grains, a usual dose. The ergotine itself is very like Liebig's meat extract in consistence and smell, and the solution in water is apt to spoil, but the mixture described is perfectly stable for a long period.

P.S.—I had written the above when I read in the Lancet a paper by Dr. Weir, where it is asked, "What are we to do in cases (hemorrhagic) of emergency, and where delay is worse than dangerous? would the hypodermic injection of ergotine be so rapid and reliable in its action as to eclipse all other remedies?" My paper answers "Yes" as regards usual uterine hemorrhages, and my experience in hæmoptysis and in the hemorrhage from the bowels of typhoid is equally affirmative.—*Lancet, Nov.* 17, 1877, *p.* 723.

84.—LIQUOR AMMONIÆ ACETATIS IN DYSMENORRHŒA.

By Clifford L. Drew, Esq., M.B.

The following case may prove interesting, as affording evidence of the usefulness of liquor ammoniæ acetatis in painful menstruation.

On June 22nd, at 6 a.m., I was called to see M.C., aged 19, who was stated to be in great pain. On arrival, I found the patient writhing in agony, her face pale, and the body covered with profuse perspiration. She is a healthy-looking and well nourished girl, but not of a marked plethoric habit; and I was informed that, with the exception of the dysmenorrhœa, her health had always been good. Since menstruation commenced, four years ago, she has always suffered more or less at her periods, but never so much as on the present occasion. I was told that menstruation had come on some little time before I was called, and that a slight "show" had appeared. I prescribed a drachm of liquor ammoniæ acetatis with a little spirit of chloroform every hour until relieved. On visiting the patient again in the course of a few hours, I was informed that she had taken two or three doses of the medicine with the most satisfactory results, and she now expressed herself as quite free from pain.

In the afternoon of the same day, the pain returned slightly, but was again relieved by the medicine, and since then there has been no recurrence.

I find that the same satisfactory results have been obtained

by my brother, Mr. C. W. Drew, previously in the same patient, and also in other similar cases where everything else has failed. How the liquor ammoniæ acetatis effects this result is not quite clear, but certainly, in some cases, it seems to act as a specific. It may be that the same influence is exerted on the uterine surfaces as on the cutaneous, increasing secretion, making the flow more free, and thus lessening the tension of the engorged vessels.—*British Medical Journal, July* 14, 1877, *p.* 46.

85.—SYSTEMATIC PREVENTION OF AFTER-PAINS.

By Dr. W. Allen Jamieson.

[After-pains are usually considered as to a certain extent beneficial.]

They are regarded as irregular uterine contractions, having for their object the expulsion of coagula contained within the womb. Granted the necessary existence of clots free in the uterus, there can be no question of the necessity of their speedy expulsion. But why should clots form there at all? Let us for a moment inquire into the conditions of the inner surface of the uterus and its walls after the third stage has been concluded. We have a large space previously occupied by the placenta, and exhibiting the openings of many torn vessels, openings to be plugged shortly by coagula. But the mechanism by which hemorrhage is prevented lies deeper. In the substance of the uterine walls at the site of the placenta are large venous sinuses as well as in the placenta itself; these have been shown by Professor Turner to be arranged in layers, and freely communicating by, as it were, a series of trap doors. These apertures are placed either alternately, or at least not opposite to each other, so that when the walls of the venous canals are approximated, each opening is roofed in and closed. This apposition of the walls is effected by the contraction of the uterus after delivery is completed, and, if absolute, would entirely prevent hemorrhage. We do meet sometimes with cases perfectly physiological; scarcely a drop of blood follows the extrusion of the placenta and membranes, and the lochial discharge is so scanty as to be quite unimportant. I have seen several such; in them the layers of uterine sinuses have been firmly appressed without so much subsequent relaxation as to permit of any considerable bleeding into the uterine cavity. This is not the common course of events, however. The uterus may at first contract firmly, but soon again partially relaxes, permits a certain degree of filling of the sinuses, and, again contracting, squeezes the blood into the uterus. The blood coagulates, and these clots set up irritation by their presence, and lead to more energetic contractions to cause their expulsion.

The above seems to me to comprise the true history of after-pains, though other causes are named in the text-books; and the remedy is to bring about *at once on completion of the third stage* such an amount of tonic uterine contraction as to prevent any refilling, however partial, of the uterine veins.

When it was taught that twenty minutes were to be permitted to elapse after the birth of a child before any attempt at removal of the placenta was to be made, any endeavour to induce immediate tonic uterine contraction would have been, from the nature of things, fruitless. But since the method advocated by Crede of squeezing out the after-birth by external pressure, has been generally adopted, there can be no objection to the employment of any safe and efficient mode of keeping the womb firmly contracted. Much may be done in this way by firm pressure by means of the hand of the accoucheur himself, and I am aware that it is the practice of distinguished obstetricians to administer a dose of some preparation of ergot by the mouth towards the close of labour. But the hand tires, and when anæsthetics are given, swallowing is impossible. In the solution of ergotin, introduced by Professor A. Simpson, we possess an agent available in every case; it is at once perfectly reliable, keeps well, and requires no preparation, while, if injected deeply over the glutæus, nothing unpleasant results from its use, save in some cases slight pain for a short time. I have now employed it in the sixteen last cases of midwifery I have attended, with the happiest results as regards the prevention of after-pains. Some of these were primiparæ, however, in whom after-pains are frequently absent altogether or slight, but three at least were marked instances of its value.

Mrs. J., aged 35, had a natural though very tedious labour with her sixth child. The head presented at the brim in the third position of Naegle and rotated into the second in its course through the pelvis. Chloroform was administered during the last two hours. After the child had been expelled and before the cord had been tied, ten minims of Dr. Simpson's solution of ergotin, were injected deep into the buttock. After removal of the child, the placenta was expelled by Crede's method, and the uterus caused once for all to contract firmly, so as thoroughly to expel all clots, then the binder without pads was applied. It was half an hour before Mrs. J. awoke to full consciousness, and she never had an after-pain to speak of, though in all her previous labours she had suffered for several days severely. During the hour or so I remained with her or in the house after the completion of the labour, I examined as to the state of the uterus and found it always hard and unyielding.

Mrs. L., aged 35, was also confined of her sixth child, after

a rapid labour, in which the head presented in the first position. No anæsthetic was given, but the same course was pursued in the management of the third stage, as in Mrs. J.'s case. As in hers the after-pains had been most severe and long-continued in her previous confinement. On inquiring as to their occurrence or not on this occasion, on my visit next day, my patient assured me with a smiling countenance that she had not had anything worth calling a pain since the last labour pain itself; not even when suckling. This last fact is specially interesting, as even in those in whom after-pains at other times are slight, the act of nursing usually evokes them.

The third case was almost similar, save that it was an abortion at the fifth month; in it there was neither after-pain nor much lochial discharge. All the sixteen cases made quick and uninterrupted recoveries.

In using ergotin in this way, there is one point to be attended to, arising out of the mode of action of ergot on the uterus. Although ergotin injected subcutaneously produces the physiological effect of the drug in the most rapid manner at present attainable, still it must be remembered that ergot acts gradually, its full influence is not manifested all at once. When given during labour the contractions brought on become by degrees more and more powerful and tetanic. So when ergotin is injected at the close of the second stage it does not at once bring the empty uterus after the expulsion of the secundines into that state of firm permanent contraction necessary to prevent after-pains. This must be done by the hand of the accoucheur himself, in the act of, and after squeezing out the placenta. The effect of the ergotin is now coming into play, and once the uterus is firmly contracted it will maintain it so. What I wish to insist on is that it will not do merely to inject ergotin and trust *to its unaided action from the first.* We must be fully assured that the uterus is contracted before we leave it to nature and ergotin.

Should more extended observations bear out to the full the value of this use of ergotin, as I firmly believe they will, both post-partum hemorrhage and after-pains ought to become, in the large majority of cases, events of the past. I believe that this treatment should be adopted in every case of labour. By its means, and whenever any lacerations occur, or even, when the second stage has been merely tedious, the conjoined use of vaginal injections of carbolic acid lotion, the puerperal condition loses much of discomfort and danger to the mother, while the anxiety of the medical attendant is lessened.

Of the value of carbolic acid injections I can speak with the greatest confidence, for I have used them in any case where there was cause for anxiety, with the result, that out of 630

consecutive labours which I have attended during the last seven years, not one patient has died within two months of her confinement.—*Practitioner, Oct.* 1877, *p.* 278.

86.—WHEN MAY WE WITH ADVANTAGE RUPTURE THE MEMBRANES BEFORE FULL DILATATION OF THE OS?

By Dr. William Stephenson, Professor of Midwifery, University of Aberdeen.

As a part of the history of our art, it is interesting to observe how exaggerated were men's ideas regarding the importance of retaining intact "Nature's wedge," and how patiently and reluctantly former practitioners would wait, under the dread of being meddlesome, for nature to do what they could readily have done, even when convinced that the non-rupture of the membranes was the cause of delay.

There is still remaining, at the present day, much of the dread of having too early recourse to this simple operation. In the face of the fact that much, and often long-continued, ineffectual exertion is often due to the integrity of the membranes, even before dull dilatation of the os, and the other fact that such ineffectual work is often productive of serious after-complications, there is certainly a want of discussion on this point in our recent works. Leishman speaks of it where there is unusual thickness and resistance of the membranes: "But before we decide on rupturing them, we should be sure that the proper function of the membranes has been effected in producing dilatation of the os." Playfair reccommends puncture before completion of the first stage only when the liquor amnii is excessive in amount; and renews the oft-repeated and considerably exaggerated caution: "If we evacuated the liquor amnii prematurely, the pressure of the head on the cervix might produce irritation and seriously prolong the labour." This latter point is a question upon which the members of this Society might with profit express the results of their experience: in how far they have observed that irritation is produced, and the labour delayed, in cases where the membranes have ruptured or been punctured before, or early in the first stage. The term irritation is vague in the extreme, and conveys no definite idea to the mind.

Before entering on the discussion of our question, it is well to define what is the exact meaning in which various terms are to be employed. By *full dilatation* of the os is meant, not obliteration, but only that degree which we know will permit the ready passage of the head; whilst the state in which the uterus and vagina are one continuous canal should be designated as *complete obliteration* of the os. The term *os* itself should be

confined to the lumen of the *cervix*, and the latter term be always employed when speaking of the state of the tissues which compose it. *Dilatation* also should be limited to speaking of the size of the os, while we speak of *expansion* of the cervix.

In reference to the puncture of the membranes, I have stated practice is at variance with teaching. Whilst our books say that this should not be done, except in rare cases, until the full dilatation of the os, many practitioners have found that by experience they can recognise certain favourable conditions, especially in multiparæ, where it is of great advantage to evacuate the waters when the os is not more than half dilated. We have seen that formerly there existed a very exaggerated idea of the function of the amniotic bag; that its purpose was supposed to be the dilatation of the whole length of the parturient canal; and that it should only be punctured when protruding at the external orifice. Modern opinion now regards the integrity of the membranes as no longer of any value after the full dilatation of the os; and it remains to be seen whether their true function should not be further curtailed, and that what at present is still empirical in practice does not rest on pure scientific grounds. The question must be answered by direct observation, and not by any imaginary views regarding the action of "Nature's wedge," the fœtal head being quite as much a wedge of nature as the bag of waters.

In discussing obstetric problems involving the first stage, it has been too exclusively the custom to take the degree of dilatation of the os, and the softness or dilatability of the tissues, as the criterion of the amount of progress made in the process of labour. This, it is easy to show, is an error; and in forming an opinion, we must take cognisance of something more. It is a matter of common experience to find that the membranes rupture spontaneously while yet the os is but slightly dilated, and that the head at once descends and comes into contact with the whole lower segment, the parturient ring being in close relation to the head. Again, it is likewise a matter of common experience that the membranes give way when the os is of the same size as in the first case, and yet the head does not come into close relationship with the parturient ring; the cervix of the lower uterine segment in this case has not in its upper part been expanded to the full diameter of the head. If the finger be introduced well through the os, it is possible to feel the head resting on a ring of firm tissue. Sir James Simpson describes this as an adventitious band of fibres which delays the first stage. It is nothing more than the unexpanded structure of the lower uterine segment. It is evident that, although the os was of the same size in both cases, yet that the mechanism of the first stage was, in the first instance, in advance of the second; and

that the difference lay in the degree of expansion of the lower segment, not in the dilatation of the os.

Next, take, what is also a matter of common experience, the condition of parts after delivery. The cervix is found hanging in the vagina, open, loosely relaxed, and elongated; while above, the walls of the uterus are firm and contracted, barely admitting the finger. From this observation (see also Matthews Duncan on Mechanism of Natural and Morbid Parturition), together with an examination of Braune's section of the frozen body of a female in the second stage of labour, it is evident that what occurs in the process of the first stage is not the mere opening up of a canal or tube which has been simply constricted in its middle; but, in addition to a constriction, there also exists a diaphragm, obstructing the lumen of the passage, and this obstruction is overcome by longitudinal as well as lateral stretching of this diaphragm. In easy labour, the constriction and diaphragm disappear simultaneously; but it frequently occurs that the disappearance of the first is in advance of the second, and the canal is dilated to its full, whilst the diaphragm has only been strained. No increase in the size of the os has taken place.

By studying the mechanism of the first stage, we can readily understand the production of these two effects of expansion and longitudinal stretching. By muscular contraction, the contents of the uterus are exposed to a uniform pressure. This force, Schulz has called the "internal uterine pressure." It is exerted on the waters, and must, therefore, be equal in all directions; and as the lower portion of the uterus is the weaker, it must yield. This, then, is the expansive force. But, as the uterus also tends to shorten itself in its longitudinal diameter, there is also a longitudinal direction given to the force, whereby it becomes expulsive. This, from the tendency of the uterus to assume its original form, Schulz terms the "form restitution power"; but, as its direction is in the axis of the uterus, I would speak of it as the *axial* force: a term more congenial to our language.

When the membranes are yet entire, this axial force can act only through the ovum as a whole, waters and fœtus; and, therefore, at a disadvantage in proportion to the quantity of the liquor amnii. When this is large, as in hydramnios, the disadvantage is at its greatest; the force, in fact, being entirely converted into the uniform internal pressure. When the relative proportion between the quantity of waters and the size of the fœtus is less, as we find it normally, then the axial force is brought to bear on the fœtus; the fundus, acting on the breech, presses the child downwards, and the head is brought to bear on the lower uterine segment. When the internal uterine

pressure is greater than the axial, the waters are forced downwards past the presenting part, which recedes. When, however, the axial force is the greater, and can act through the fœtus, the contrary effect results; the water is forced upwards, and the head is brought into close proximity with the lower portion of the uterine walls. When the child is thus forced down during a pain, the uterine walls closely surround the head, and the membranes being still entire, the liquor amnii is divided into two portions; that in front of the head is called the forewaters. If the division be complete, then the entirety of the membranes is really a disadvantage; for now the forewaters but impede the more powerful action of the axial force. If the separation be incomplete, then the expansive action is only obtained, the internal pressure being still in excess of the axial. If the reverse be the case, the forewaters are but forced back above the head. By the mode of action, the internal uterine pressure is the force which tends to expand the lower uterine walls. Acting, in fact, like a glove-stretcher, its expulsive power can only act on the entire ovum, and is, therefore, at a disadvantege. The axial force is exerted mainly through the fœtus, and can exert its full strength only after the membranes are ruptured.

It seems, therefore, evident that *that the function proper of the bag of waters should be limited to that of expansion only.* But the full dilatation of the os is effected, not by expansion alone, but also by longitudinal stretching. When, therefore, we find dilatation tardy from defect in degree or direction of the power alone, and not from any inherent character of the tissnes, when once it is evident that the lower segment of the uterus is well expanded, the rupture of the membranes is the most effectual means of favouring the dilatation, by bringing the axial force into full action, and this irrespective of the degree of the size of the os.

By the researches of Dr. Matthews Duncan on the Power of Natural Labour, a beginning has been made to place this subject on a more purely scientific and accurate basis; but we are not yet in a position, and it requires qualifications which few possess, to follow up the subject as he has done. He has, however, shown mathematically, what has been long practically known, that partial evacuation of the liquor amnii is an efficient way of improving the power of the uterus, even when defective in amount. "It is a common belief," he says, "that the uteriue pains increase in strength after the evacuation of the liquor amnii. Whether this be true or not, as commonly believed, I do not here consider. But it is certain that, if the uterine contractions remain of the same force after as before the partial evacuation of the liquor amnii, the power of the labour or the extruding force will be increased, as the curvature of the con-

tracting organ is increased." (Researches in Obsterics, p. 315.)

Having laid down the basis of our knowledge, it remains only to discuss the diagnosis of the conditions which warrant us in having recourse to rupture of the membranes before the full dilatation of the os. The first point is the determination of the degree of expansion of the lower uterine segment. We have seen that the size of the external os is no criterion of expansion. The os, in fact, may be very small, and yet expansion may be complete. It is by the internal os that we can best judge, but this is hard to reach, and difficult to determine its exact site. There is one means, however, of ready access, wherehy we can form a proximate opinion: it is the degree of dilatation or updrawing of the vaginal *culs-de-sac*. This is a point which has been entirely left out in the consideration of the progress of the first stage. It is a matter of common experience to find, in the class of cases where we feel something is required to promote a labour with tardy dilatation of the os, that the upper part of the vagina is well expanded and drawn up, greatly increasing the perceptible diaphragm of the cervix, which alone obstructs the continuity of the developed canal. Now, we know that the longitudinal muscular fibres of the vagina run upwards, and are continuous with those of the body of the uterus. and that the attachments of the uterus in their upper portion correspond with the internal os. This portion, then, cannot undergo expansion without carrying with it the tissues which are in connection therewith. Consequently, we find that, as the first stage of labour advances, the upper part of the vagina is dilated until it seems to coincide pretty closely with the upper part of the bony canal. When, therefore, a considerable portion of the lower segment of the uterus can be felt *in* the vagina, and not merely *through* its walls, expansion is certain to be complete, whatever may be the size of the parturient ring; and the tissues composing it are those of the cervix proper, and not the uterus. Under such circumstances, I believe the membranes may be ruptured with advantage. It is, however, unnecessary in many cases to wait for the full development of the condition above described. I have taken the extreme state as being most readily understood, and indicating the direction in which our observations should be made.

Another class of cases, or it may be only an additional character to those of the first, are where the action of the uterus seems to be effecting, not steady dilatation, but extreme thinning of the tissue of the cervix; and also where the head is felt to be in close contact with the parturient ring, there being little or no bag of waters.

The next point to be considered is the quantity of liquor amnii; not the actual quantity, as is generally referred to when

speaking of it being present in excess, but the proportion its amount bears to the size of the child, and also to the capacity of the amniotic sac. This latter is rarely quite filled; otherwise, it would remain much more tense than it usually does in the intervals between the pains. If it be nearly or entirely distended, it will interfere with the power of restitution of form, by preventing alteration in the form of the uterus, and consequent action on the fœtus, even though the actual quantity of waters is not greater than ordinary. In this circumstance, it must be regarded as really in excess, quite as much as where there is excess in actual quantity. Undue tension, therefore, of the membranes *during a relaxed state of the uterus* must be regarded as unfavourable to the mechanism of labour, and as warranting an earlier rupture of the membranes than under other circumstances.

The liquor amnii must also be considered in excess, irrespectively of actual quantity, if it be unduly great in proportion to the size of the child. Here, again, it interferes with the action of the force which restores form, or the axial force. If, therefore, the parts of the child be not recognisable externally with ordinary facility *during a relaxed state of the uterus;* if *ballottement* be unusually facile, and especially can be felt during a pain, the probability is that there is a true excess of liquor amnii; and this condition would fully warrant the rupture of the membranes before the full dilatation of the os; the other conditions being favourable to the operation.

I have discussed the subject apart from the state of rigidity or dilatability of the cervix, conditions which undoubtably must be taken into consideration in determining any line of treatment in the first stage; but the subject of rigidity is one which requires discussion by itself, and would only tend to complicate and obscure the question.—*British Medical Journal, Aug.* 4, 1877, *p.* 132.

87.—ON DRAINAGE IN OVARIOTOMY.

By Dr. George Granville Bantock, F.R.C.S.Ed., Surgeon to the Samaritan Free Hospital for Women and Children.

With the exhaustive works of Spencer Wells and Peaslee in our hands, it might be thought that there remained nothing new to be said on the subject of ovariotomy. There is one point, however, to which attention may be profitably drawn, and on which you will look in vain for any information in these works. I refer to the subject of drainage of the peritoneal cavity as one of the steps of the operation.

It may be said that most of the early operations in which the ends of the ligature were left hanging from the wound were

examples. This arose from the erroneous ideas then prevailing as to the behaviour of a portion of ligatured tissue. It was universally believed up to a late period, and it is even now a matter of extensive belief, that sloughing of the distal portion must ensue; and it is only within the last few years that it has been shown, both by direct experiment and by *post-mortem* examination of persons who have been subjected to ovariotomy, that, if the ligature be cut off short and guarded against the putrefactive process, such a result is almost unknown.

In 1872-3, Dr. Marion Sims published a paper in the New York Medical Journal, in which he showed, from an analysis of a large number of *post-mortem* examinations, that, in the great majority of cases, death was due to the absorption of septic matter from the peritoneal cavity. The conditions were found so similar, and the symptoms preceding death presented such uniformity, that no other conclusion was possible. He further showed that, as in Mr. Spencer Wells's hands, this usually fatal train of symptoms had been checked by the evacuation, through the vagina, of a quantity of highly irritating and often offensive fluid. Hence, he argued, that if we could only prevent this accumulation, we should obviate a considerable number of deaths. To this end, he ventured on the heroic proposition "to puncture the *cul-de-sac* of the vagina behind the cervix uteri, and to pass a tube of some sort into the peritoneal cavity to drain off any effusion that may take place in the said cavity," and "to do this in every instance, whether there are adhesions or not." In illustration, he quotes five cases in which this proposition was acted on, and of which three were successful. On perusing these cases, one cannot be favourably impressed with the results, which are far from justifying the universal adoption of this procedure.

At a recent meeting of the Royal Medical and Chirurgical Society of London, Mr. Spencer Wells gave an analysis of "three hundred additional cases of ovariotomy," in which he did little more than allude to the subject of drainage. Nowhere, as I have said, will you find any detailed information on this subject, and we are thrown back on a few isolated cases which are difficult to find.

It will be my duty, then, to speak, first, of the cases that would appear to demand the application of this principle, with illustrative examples; secondly of the modes of effecting the object in view; and thirdly, to endeavour to indicate the most effective and least troublesome method.

First, as to the cases which appear to demand this proceeding: —I may at once say that the idea of draining the peritoneal cavity after a simple uncomplicated case, *i.e.*, without adhesions and with a good pedicle, is not to be entertained. Speaking

generally, I would say that whenever, in the course of operation, it becomes evident or probable that oozing of blood or serum, or both, must go on after closure of the wound, and especially where there has been an escape of ovarian fluid into the peritoneal cavity, then drainge will be demanded. This general proposition will be best enforced by illustrative cases. It has probably occurred to those who are in the habit of seeing, or have seen, much of ovariotomy, to regret that means had not been used to avoid that which has been revealed by a *post-mortem* examination. This, at least, is my experience; and I cannot but regret the omission in the first case which proved fatal in my hands. The operation was performed under very unfavourable conditions. I had tapped the patient a week before. I removed twenty-three pints. This was followed by inflammation of the cyst. On the morning of the operation, the patient's temperature was 101 deg. She died in four days of septicæmia. There was about a pint of very acrid serum with broken down blood in the peritoneal cavity. The clamp was used. In some cases, the pedicle has served as a drain, as in a similar case which I saw under Mr. Wells's care. Had I used the drainage-tube, it is probable the result would have been different.

Case 1.—J. S., aged 25, was admitted into the Samaritan Free Hospital in February 1876, under the care of Mr. Spencer Wells. A week afterwards, she was tapped. Inflammatory symptoms followed, but she recovered within three weeks, when she went to a convalescent home. She returned again on July 6th. The patient had lost much flesh; tumour very multilocular; adhesions evident; uterus drawn up almost out of the pelvis; altogether an unpromising case. Mr. Wells kindly offered me one of his cases, and I selected this as the most urgently demanding operation. I had arranged to operate at 9.30 a.m. on July 21st, 1876. On my arrival, a little before that time, I found that the patient had been seized some hours before with severe abdominal pain. She was then too tender for examination, but the tumour had lost some of its prominence; there was more fluctuation (superficial). Pulse 130; temperature, 104·4 deg. It was believed that rupture had taken place; yet I decided to proceed with the operation as a forlorn hope.

Operation.—On opening the peritoneum, a considerable quantity of ovarian fluid and thin colloid matter escaped. The tumour was first tapped and then broken up with the hand. As the mass was drawn out, the adherent mesentery was ligatured as it came into view, as well as one of the fimbriæ of the left Fallopian tube. Abundant, but not very firm, pelvic adhesions were then broken down, liberating the mass, which sprang

from the right side by a pedicle about an inch long and two inches broad. The pedicle was transfixed and secured by a double ligature, of which the ends were cut off short. A long tail of omentum was found adhering to the pedicle close to the uterus. Two ligatures were applied, and division effected between. A portion of omentum adhering to the abdominal wall close to the incision was not interfered with. It required a great deal of sponging to clear away the serum and colloid matter from the pelvic cavity and from amongst the intestines, and considerable oozing of bloody serum went on. Under these circumstances, I deemed it advisable to put a glass tube through the bottom of the wound, reaching to the bottom of the utero-rectal cul-de-sac before closing the wound with seven sutures in the usual way.

An hour after the operation, the temperature was 100·8 deg., and the pulse 146. Eight hours afterwards, the temperature was 102 deg., and the iced water cap was applied. Bloody serum escaped, and was drawn off from the tube for four days —in all, about a pint; in the course of the fourth day, a piece of India-rubber tubing was substituted for the glass; and on the next day, the discharge having ceased, this was removed and the opening forthwith closed up. The patient left her room on the eleventh day, and the hospital on the twenty-fourth day.

Case 2.—M. H., aged 48, mother of four children, was admitted into St. Bartholomew's Hospital in August 1875, under the care of Dr. Greenhalgh. A few days afterwards, the tumour burst; the fluid was absorbed; and at the end of three weeks, the patient left the hospital very much reduced in size. She re-entered the hospital in June 1876, and on the 26th Mr. Thomas Smith removed twenty-one pints of fluid by tapping. The case was considered too unfavourable for operation; and on August 14th she entered the Samaritan Hospital, where I tapped her for Mr. Wells, removing twenty-two pints, and leaving a larger mass than before. The hospital was about to be closed for the annual cleaning, and the patient was sent home. At the reopening of the hospital on October 1st, the patient returned, urgently requiring relief, and Mr. Wells kindly allowed me to keep the case. On October 4th, she measured 42 inches in circumference at the umbilicus, 9½ inches from the ensiform cartilage to the umbilicus, 10½ inches from the umbilicus to the symphysis pubis, 11½ inches from the umbilicus to the right anterior superior spine of the ilium, and 12 inches to the left. I tapped her of nineteen pints and a half of fluid to relieve the dyspnœa and pain of distension. There remained a larger mass than before firmly adherent on the left side (groin); the uterus was drawn up above the pubes, behind which the os could just

be felt, and the roof of the vagina was much stretched and funnel-shaped. It was impossible to make out the outline of the uterus, which appeared to be merged in the tumour. After consultation with Mr. Wells and Drs. Savage and Greenhalgh, it was decided to give the patient the benefit of the doubt —a doubtful benefit. Apart from the vaginal condition, the difference in the external measurement, indicated extensive adhesions.

On October 11th, in the presence of Drs. Greenhalgh, Junker, and several others, my colleague Mr. Thornton assisting me, and Dr. Champneys administering bichloride of methylene, I made an incision five inches long in the usual situation. The tumour was found universally adherent anteriorly, and some difficulty was experienced in separating the peritoneum and cyst-wall. After breaking down the adhesions and setting the tumour free, the uterus was seen to be embedded in the mass, and raised above the level of the pubes. Three ligatures were applied to as many pieces of adhering omentum, and the tip of the appendix cæci was secured in the same way. The broken up tumour was drawn out as much as possible, but it obstinately remained adherent deep down in Douglas's pouch, and I was in doubt as to the possibility of separating it. While holding the mass on the stretch with the right hand I pressed the tips of the fingers of the left on what appeared to be the line of union. It began to give way, and I went on peeling off the tumour from the posterior aspect of the broad ligaments and uterus until I reached the fundus uteri. Here the uterine tissues proper were torn, and free bleeding ensued. Having passed beyond the fundus, I broke through the cyst-wall where it was thin and non-vascular, and, having applied a ligature to each cornu, I cut the mass away. There now remained a flap of cyst-wall on each side, about the size of the palm of the hand. A deep fissure in the fundus, from which there was rather free bleeding, was brought together by a continuous suture of fine silk, and several small bleeding points were secured by fine ligatures. The left flap was cut away so as to fit on the back of the uterus, to which it was fixed by sutures along the fundus, small bleeding points having been secured as they appeared in the cut edge. The right flap was likewise cut away as low as possible, care being taken to secure some small vessels as they appeared. The peritoneal cavity was now cleared out, and there being no active bleeding, though considerable oozing, I passed a glass tube into Douglas's pouch and closed the wound. The operation lasted an hour and a half; and, from the loss of blood and the severe character of the operation generally, it was the universal opinion that the patient had only a few hours to live.

Five hours afterwards, the bandage and dressings were found saturated with dark bloody serum, and about an ounce of almost pure blood was drawn from the tube. Temperature 100·7 deg.; pulse 104; respirations 30. Next morning, in addition to the contents of the sponge, which was saturated, I drew from the tube about two drachms of dark bloody serum. At the end of two days and a half, I obtained a small blood-clot. Temperature 103 deg. The iced water cap was put on. For the next hour the temperature continued to rise, until it reached 103·6 deg. In fourteen hours, it stood at 99·8 deg., and the cap was removed. The temperature again rising, it was reapplied; and, with several changes, it was finally removed on the fifth day. The discharge from the tube continued, though in gradually diminishing quantity; portions of clot were obtained on the fourth and fifth days; on the sixth day, only about a drachm of pale red serum was obtained at each dressing, and the glass tube was withdrawn, its place being taken by a piece of India-rubber tubing of about half the diameter. This was removed the next day. On the eighth day, the opening was closed; on the eleventh day, the patient was outside the bed; and on the seventeenth day, she went to a convalescent home. She is now in excellent health.

Case 3.—Mrs. C., aged 27, mother of four children, the youngest of whom was under two years, was admitted into the Samaritan Free Hospital on October 31st, 1876, under my care, with a very large multilocular tumour. She measured 44½ inches in circumference at the umbilicus, 13½ inches from the ensiform cartilage to the umbilicus, 9 inches from the umbilicus to the pubes, and 11 inches from the umbilicus to the anterior superior spine of the ilium. (I may here again call attention to these differences of measurement as a sure indication of parietal adhesions.) The tumour was exposed by an incision five inches long, afterwards extended upwards to about eight inches. Adhesions were very firm over the whole anterior and lateral aspects of the tumour, from a little above the pubes to the ensiform cartilage. They were broken down with difficulty. On the right side, near the brim of the pelvis, there was a band of adhesion about an inch and a half broad, which I could not break through. It was ligatured. Four ligatures were applied to as many portions of adherent mesentery; and the pedicle, which was from two to three inches long, was treated in the same way. The tumour weighed fifty pounds, and the operation occupied about an hour. In consequence of the very extensive adhesions and the continuance of oozing, I deemed it advisable to use a drainage-tube. About thirty ounces of serum, at first bloody, finally becoming colourless, were obtained; the tube was removed after four days and three quarters;

the wound healed well; the opening closed within thirty-six hours after the removal of the tube, and the bowels acted on the eighth day. Her convalescence was retarded by the appearance of a large induration above and to the left of the umbilicus, threatening an abscess in the abdominal walls. It, however, gradually disappeared under treatment, and the patient left the hospital at the end of a month. She is now quite well.

Case 4.—Miss C. U., aged 27, was admitted into the Samaritan Free Hospital on January 1st, 1877, with a multilocular ovarian tumour of about two years' growth. On the night of January 5th-6th, the cyst ruptured. This was followed by symptoms of peritonitis. On the 8th the temperature was 104 deg., and the pulse 142 at 9 a.m. For a fortnight, the patient was so ill that death was feared; the abdomen became very tympanitic, tongue dry, red, and glazed, with troublesome sickness and obstinate constipation; and she lived on small quantities of milk, beef-tea, chicken-broth, gruel, and latterly a little bread and butter as the most solid food.

On the eighteenth day, her morning temperature was 98.6 deg., but the evening temperature rose to 102 deg.; and at 9 p.m., it stood at 101 deg.; pulse 120; the tongue had become a little moist; the bowels had been well moved by frequent small doses of colocynth and henbane; but the tympanites remained. A line drawn from the right hypochondrium to the inner end of the left Poupart's ligament, pretty nearly straight, divided the abdomen into two parts, of which the left was very tympanitic, and the right wholly dull on percussion. I determined to give her the chance afforded by an operation, as otherwise her death could not be far off. The patient readily assented to my proposition.

Nineteenth day, 9 a.m. Temperature, 99.4; pulse 112. Noon temperature, 100 deg. The operation was begun at 2.45, in the presence of Drs. Gage Brown and Baxter Forman, who were interested in the case, and several other gentlemen. My colleague Mr. Thornton assisted me, and Dr. Champneys administered the anæsthetic. On dividing the abdominal wall, I found the peritoneum and cyst-wall intimately adherent, and I had to open the tumour at once, giving exit to a dark-brown viscid fluid. Having then enlarged the opening and evacuated the greater portion of the fluid contents, I succeded in separating the cyst-wall from the peritoneum for about an inch in breadth on each side. On these flaps, I fixed a pair of vulsellum-forceps, by which I was able to steady the tumour while I introduced my hand and broke up the mass. This done, I then proceeded to break down the parietal adhesions, which were universal, recent, and very vascular. A small piece of omen-

tum was secured by ligature as soon as it came into view. I now passed my hand behind and around the tumour, rupturing adhesions to the intestines until it was set free. The pedicle, about two inches long and rounded, cord-like, about the thickness of a medium sized finger, was secured by a single ligature, and the mass was cut away. The transverse and descending colon, considerably distended, was now seen to be adherent to the abdominal wall, in a diagonal direction from right to left downwards, and close to the umbilicus and the top of the wound. From this down to the pelvis, an unbroken surface was presented, like the inside of a cocoanut-shell, formed by the adhesion of coils of small intestine to one another and to the colon, where they lay in contact with the left posterior aspect of the tumour; and the floor of the cavity was formed by the adhesion of the uterus and right broad ligament to the adjacent structures. Out of the floor of the cavity thus formed, I took a handful of firm colloid matter, having a very peculiar odour. High up on the right side, I could get amongst the intestines, which, although adhering to the tumour, were free from one another. To a less extent, I could get down into the left iliac fossa. Douglas's pouch was obliterated. There was free oozing from the surfaces of the intestines where the adhesions were ruptured, and one point near the bottom of the cavity required a fine ligature. A glass tube was finally inserted, and the wound closed in the usual way with seven sutures. The tumour weighed eleven pounds.

Two hours after the operation, the temperature was 101·4 deg., and pulse 120. In six hours, the temperature had risen to 102·8 deg., pulse 144, respirations 24; and the iced water cap was put on. The bandage and dressings were saturated with red serum, of which I obtained about an ounce and a half from the tube and sponge. In twelve hours, the dressings had to be changed again, but the discharge was less free. Temperature 101·6 deg.

Suffice it to say that the discharge rapidly diminished, becoming paler; flatus passed in about thirty hours; the tube was removed after sixty-five hours; two stitches were removed on the third day, and all were out on the sixth, the wound quite healed. The iced cap was kept on continuously for about fifty hours, when the temperature had fallen to 99.2 deg., pulse 103; it was resorted to for short periods for four days more, and was finally removed with the temperature at 98.6 deg. The bowels acted on the seventh day, after taking pills of a grain of compound extract of colocynth and one-sixth of a grain of extract of belladonna, and having an enema of soapy water. The patient was out of bed on the fourteenth day, and left the

hospital on the twenty-sixth day, walking as sprightly as if nothing had happened.

Case 5.—M. A. W., aged 15, was sent from the camp at Aldershot by Staff-Surgeons A. F. Churchill and Alcock to the Samaritan Free Hospital on January 12th, 1877. Her temperature at 5 p.m. was 101·6 deg.; pulse 136. She looked much under age, and was greatly emaciated. There was great tenderness in the tumour, which felt very solid, and moved freely in a considerable amount of ascitic fluid. The circumference at the umbilicus was twenty-eight inches. The skin was harsh and dry; the urine pale and free. On the third day, I gave her five minims of tincture of perchloride of iron three times daily; and, after some days, I increased the dose to ten minims. The urine became much more free, from three to four pints daily, very pale; specific gravity 1008; no albumen. At the end of a fortnight, the ascitic fluid had disappeared; the tumour was but slightly tender; the maximum daily temperature exceeded 100 deg.; and on the eighteenth day, at 9 p.m., it was 100·6 deg.; pulse 140. It was now evident that no more improvement could be expected, and that since the disappearance of the ascitic fluid, the tumour was becoming adherent to the parietes. I, therefore, decided to operate next day.

January 31st, 9.30 a.m. Bichloride of methylene was administered by Dr. Percy Boulton, and Mr. Thornton assisted me, in the presence of several visitors. As I anticipated, the tumour was found universally adherent to the parietes and adjacent structures. The adhesions were evidently recent, as shown by their friability and excessive vascularity. The trocar thrust into the tumour drew no fluid, and I had to enlarge the incision to about five inches. The tumour was now turned out entire, bringing into view a thick pedicle about two inches broad and three inches long. The pedicle passed into the tumour half way up its left face, and appeared as a broad thick band, non-adherent, though in close apposition. A double ligature was applied, and the mass was cut away. A coil of small intestine adhered to the abdominal parietes just above the level of the left iliac crest. There was considerable sanguineous oozing from the ruptured adhesions in Douglas's pouch. The insertion of a drainage-tube and the closure of the wound by ten silk sutures completed the operation. The tumour was dermoid, and from it I obtained these specimens of bone. (The specimens of bone were shown.) There had been hemorrhage into one or more of the larger cysts situated at the back of the tumour; and this was, no doubt, the cause of the inflammatory action set up.

Half-an hour before the operation, the temperature was 99·6 deg. (pulse 140); and an hour after, it was 99 deg. In six hours I removed the dressings, which were saturated with red serum, and three drachms of dark bloody fluid were obtained from the tube. The temperature had then risen to 102 deg., and the iced water cap was put on. An hour later the temperature was 102·4 deg., and the pulse was 172, and respirations only 16. For eight hours, the temperature stood steady at 102·6 deg.; but in two hours later it had fallen to 101.8 deg., and it continued to fall until at midnight (thirty-two hours) it stood at 100 deg., and the pulse had fallen to 132. The temperature again rose gradually in the next ten hours up to 102.3 deg., with a pulse of 148; and during eleven hours it fell as slowly to 100 deg. From this time, it gave no more anxiety, but the pulse would not fall below 130, and often ran up to 144. The iced water cap was finally removed on the fifth day. In all, about half-a-pint of serum escaped; the tube was removed after sixty-seven hours, when the serum was of a pale brown colour and inodorous. None escaped after this, and the opening closed in a few hours. Several small clots were removed from the tube in the process of emptying it. On the third day, she passed six pints and a half of urine of specific gravity 1008, of alkaline reaction, and containing one-fourth albumen. The subsequent history of the case does not bear on the question under consideration; and I have only to say that, in consequence of the formation of an abscess in the wound and the condition of the urine, the patient was not able to leave the hospital till the forty-first day. She is now in very fair health.

These five cases, presenting such varying and unpromising conditions, abundantly prove the great service rendered by the drainage-tube. I do not hesitate to say that, but for this, the result must have been different. I call particular attention to Case 2, in which the whole posterior surface of the uterus was in a raw state, with not less than a dozen ligatures, large and small, in its immediate neighbourhood. It was the general impression in the room that this patient had only a few hours to live; but we have seen that, by the early removal of the blood and serum, she was able to make an excellent recovery. But the most remarkable recovery was that of Case 4, in which the intestines were glued together by recent peritonitis.

It is now time to speak of the mode by which the drainage is effected. Dr. Marion Sims recommended that the drainage should be effected through the recto-uterine *cul-de-sac* and vagina; but the perusal of his cases will not impress one favourably. The difficulty of keeping the tube pervious and

of preventing putrefaction is almost insuperable. If we could contrive a method by which we could convert the recto-uterine pouch into the form of a funnel, no doubt this would prove the most efficient. But at present I am unable to see my way to this. Dr. Marion Sims's cases are not encouraging, for they show us the great difficulties attending it, even in his skilful hands. He figures an instrument, like a bivalve speculum, which he recommends, but which he had not then submitted to the test of experiment. Kœberle was, I believe, the first to employ a glass tube communicating through the lower end of the wound with Douglas's pouch. This tube was closed at the lower extremity, which in shape was rounded and pointed, and it was pierced throughout its length with a number of small holes. The latter I always regarded as a mistake, as it was not only inconvenient for removing the fluid collected in the tube, but admitted air too freely, and did not confine the fluid. It is to Dr. Keith that we are indebted for the form of tube which I now show you, and which appears to me to answer every purpose. It is made of various sizes as to length and diameter, and its peculiarities are that it is pierced for about an inch only of its lower end, that this is open, and that at the other end it has a shoulder to prevent its slipping into the peritoneal cavity. The fluid is thus drawn from the lowest part of the peritoneal cavity, and the air comes into contact only with the surface of the fluid in the tube.

To my mind, this tube is at once the most simple and efficient method yet attempted. It is managed in this way. After all actively bleeding points are secured and the sutures are all passed, Douglas's pouch is finally cleaned out, and, while with one hand the intestines are kept out of the way and the fingers serve to guide the tube, it is passed down to the bottom of the pouch between two of the sutures. Care must be taken that the tube maintains a perpendicular position. The sutures are now tied, a cap-shaped sponge wrung out of a solution of carbolic acid (one to twenty), is placed over the tube, and the dressing is completed. It is important to perform pressure uniformly over the abdomen, so that the fluid as fast as it is poured out is forced into the tube. Sickness soon after the operation is rather desirable than otherwise, as it prevents partial collections which are very apt to form from the union of raw surfaces which is inevitable in the case of torn adhesions. Immediately on the appearance of any discharge in the dressings, these should be changed. Under any circumstances the first dressing should be done within from four to six hours. The sponge is well washed out and recharged with carbolic acid, and the contents of the tube are removed by means of a glass syringe, over the nozzle of which is drawn a piece of India-rubber

tubing a little longer than the glass tube. The frequency of the dressing after this will depend on the amount of discharge; but it will be well to repeat it at intervals of six or eight hours. At first, the serum contains more or less blood; but gradually becomes paler and paler, and diminishes in quantity. As soon as the discharge becomes of a pale colour and is so far reduced in quantity that not more than half a drachm or a drachm can be obtained from the tube and none has overflowed into the sponge, the tube may be removed. This usually happens about the fourth or fifth day. It is a good precaution to substitute a piece of India-rubber tubing for a few hours. If no fluid be obtained, or only a few drops, this also may be removed. The opening now closes in a few hours. Should, however, the general condition of the patient be in any degree unsatisfactory, it will be well to wait until the symptoms have subsided. The longest period to which the drainage was carried on in these cases was five days. This contrasts very favourably with Dr. Marion Sims's cases. In his first case, the drainage was continued for a fortnight; in the second, for six weeks; in the third, the drainage was a failure, chiefly from the mode adopted; in the fourth, for about a month; and in the fifth, it again failed.

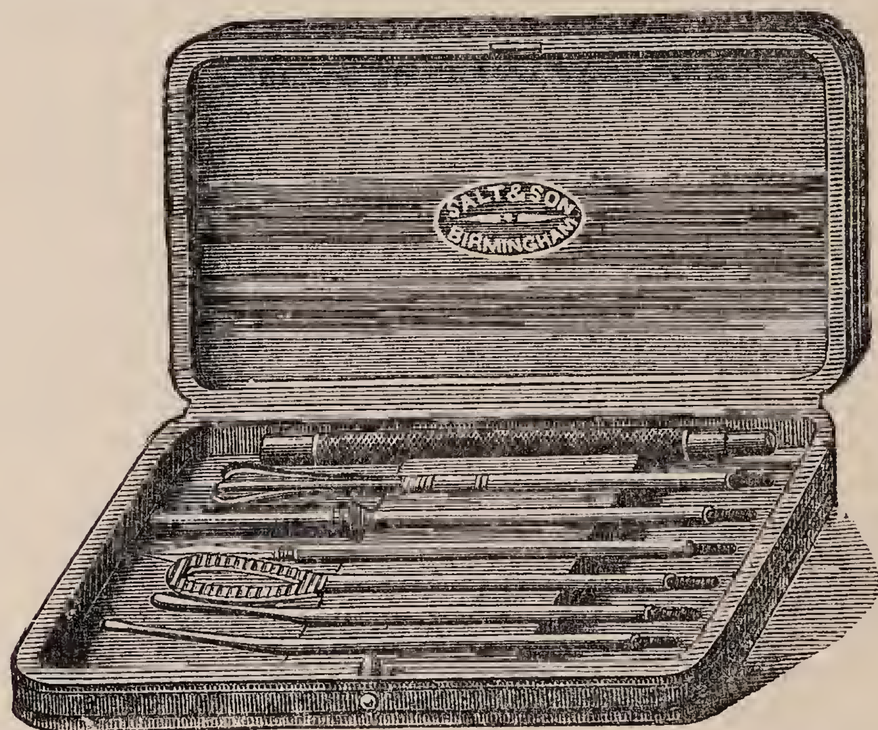
One great difficulty in drainage through the vagina will be that of preventing putrefaction. In not one of my cases was there even an approach to this.

It is thus an easy matter to insure drainage of the utero-rectal pouch; but the difficulty is to prevent partial accumulations. Since the above was written, I have had another case in which a collection of bloody serum took place between the liver and the parietes. The case was one of suppurating ovarian tumour intimately adhering to the parietes extending high up between the liver and the ribs. There was no sickness after the operation. On the third and fourth days, the temperature was normal, and the tube was removed on the third day. On the fifth day, the temperature began to rise; and she died within twenty hours, with all the symptoms of septicæmia. The *post-mortem* examination showed that the drainage of Douglas's pouch was perfect, and there was no indication that the tube had been used. The liver was adherent to the raw surface in apposition, and behind this there was collected a small quantity of decomposed bloody serum, under two ounces, which had produced death. But for this small collection, the result would have been perfect, as shown by the examination.

How to prevent these partial accumulations is the only remaining difficulty; but this paper has already attained such a length, that I must not further detain you.—*British Medical Journal, Sept.* 29, 1877, *p.* 436.

88.—PORTABLE UTERINE INSTRUMENTS.

We have examined a very handy and complete set of instruments for uterine purposes submitted to us by Messrs. Salt and Sons, surgical instrument makers, Birmingham, which we think will be found extremely useful. The instruments, six in number, the handle being made of aluminium beautifully finished, are contained in a neat case six inches long by three inches wide and three-quarters of an inch thick. They are, a *porte-caustique*,



lancet, sponge-holder, Playfair's probe, Simpson's sound and uterine elevator. These instruments all fit the same handle, and are propelled to the required length by a rotating motion similar to the clinical thermometers recently introduced by the same firm. The total maximum length of each instrument is twelve inches—long enough for use with any speculum. This pencil-case motion reduces the size of the case by one-half, and thus renders it specially convenient for the pocket.—*British Medical Journal, Feb.* 24, 1877.

ADDENDA.

89.—ON THE ACTION OF SALICINE.

By Dr. A. D. L. Napier.

Since July 1876 I have used salicylic acid and salicylate of soda very frequently; in fact, I have treated almost every case of rheumatism, of an acute or subacute nature, which has come under my care, more or less, by means of these drugs. The smallest dose prescribed was three grains of salicylic acid every three hours, the patient in this instance being a child of six years old; the largest dose prescribed to adults was twenty grains every two hours for sixteen hours.

The form of rheumatic disease for which I have most frequently ordered salicine, is the arthritic, and in these cases relief was almost invariably experienced. In one case of severe arthritis of the left finger, wrist, and ankle joints, decided benefit attended the exhibition of a fifteen-grain dose, and though the disease was of six days' standing, complete relief from pain was experienced after other three doses. In such cases I have repeatedly seen reduction of pain, redness, heat, and swelling about an hour and a half after the administration of a twenty-grain dose.

The salicylate of soda, in addition to its general action in lessening arterial tension, acts frequently as a powerful diaphoretic, producing increased perspiration, large flow of urine, and in some cases an increased quantity of saliva. These latter effects seem to be more often caused by the soda salt than by the acid. Although swelling frequently is materially decreased in a short time by salicine, yet in some cases this is not so: I have a patient at present, who suffered from rheumatic arthritis of the wrist joint, was treated by salicylate of soda, and relieved of all acute pain, more than a month ago, whose joint is still greatly swollen, and useless for all active exertion; he is now rapidly improving under galvanism.

Symptoms exactly similar to cinchonism may follow the prolonged use of salicine. An old gentleman who was under my care suffering from rheumatic affection of the wrist and ankle joints, was ordered twenty grains of salicylate of soda every two or three hours; a few doses speedily cured him. He ceased taking the drug, and was again similarly affected, about ten days after his first attack; the drug was resumed, and he was recommended to continue it for a fortnight, in ten-grain

doses twice daily, after all symptoms had disappeared. He only used it however for two or three days. Within a short time he again became ill, and having experienced the decidedly beneficial action of his former medicine, resumed taking it without sending for medical advice. On this occasion, evidently desiring to make assurance doubly sure, he persevered in taking twenty grains every three hours for more than a week, although the pain had almost ceased after two or three doses. He then became very deaf, had ringing noises in the ears, experienced severe headache, thirst, loss of appetite, and felt dull and heavy. The medicine was discontinued, and the unpleasant symptoms shortly vanished. It is necessary for the perfect action of salicine, that the drug should be used in reduced doses for some time after acute symptoms are dispelled; I have often seen a relapse from a too early cessation of the medicine.

In muscular rheumatism, salicine affords some relief, but its action in such cases has given uncertain results in my hands. In neuralgic affections, I have seen good from salicylic acid, more especially in mixed cases of neuralgia and rheumatism; one case of neuralgia of the brachial plexus was undoubtedly cured in a very short time. From its greater solubility, and from its being more easily taken by the majority of patients, I have found salicylate of soda preferable to the salicylic acid. With the exception of the greater diaphoretic action of the former, I have been unable to discriminate between their therapeutic action.—*Practitioner, June* 1877, *p.* 410.

90.—CHLORIDE OF CALCIUM AS A THERAPEUTIC AGENT.

By Dr. J. G. S. Coghill, Royal National Hospital, Ventnor.

I have for many years both in my own family and in practice, employed a solution of the chloride or muriate of calcium in certain cases, in which I have come to regard it as possessing quite the character of a specific. Previous to the year 1825, when iodine and its salts were introduced by Coindet, Lugol, and others as *the* remedy for strumous diseases, the profession trusted almost exclusively under similar conditions to the chloride of calcium, commonly known as the muriate of lime, and which must not be confounded with the chloride, more properly the hypochlorite of lime, or the bleaching powder of commerce.

The almost invariable tendency of iodine and its salts to occasion inconvenience from the development of their physiological symptoms, their proneness to cause dyspepsia and disturbance of the functions of the mucous surfaces generally, and

above all their liability to induce, when long continued, emaciation, and also absorption, more especially of the osseous structures, greatly impair their otherwise valuable properties, and limit their employment, particularly in young persons. It is of course in the latter we are so often called upon to treat the various outcomings of the strumous diathesis, and it is precisely in this class of ailments that the chloride of calcium will be found to possess therapeutic properties, which render it altogether a much more potent, and certainly more manageable remedy than any of the various preparations of iodine.

It is not only when glandular enlargement proclaims that the scrofulous cachexia has been developed, that chloride of calcium is of service. In those premonitory derangements of the digestive and assimilative processes, which in delicate and predisposed constitutions so constantly precede or favour, if they do not indeed directly cause, the appearance of the more characteristic lesions of the strumous diathesis, the chloride of calcium is of the greatest use. In children and young persons when the sleep becomes restless and troubled, the breath fetid, the tongue foul and coated, the tonsils enlarged, the evacuations irregular and offensive, with deficient secretion of bile, I know no remedy approaching it in value. The colliquative diarrhœa, which so often accompanies this condition, and above all that obstinate lientery which is seen with hypertrophy of the mesenteric glands, yield to the solution of the chloride of calcium like a charm. In pronounced tabes mesenterica, the usual sequela to the condition above described when neglected, it is equally efficacious. I have also found this remedy of great use in controlling and modifying the characteristic alvine discharges in enteric fever. In fact turpentine stupes to the abdomen, and the solution of the chloride of calcium in milk internally, are what I have for long mainly trusted to in the treatment of the milder uncomplicated forms of typhoid.

It is important to remember, that it should always be made of the crystallized or hydrated chloride of calcium, as the ordinary crude anhydrous salt gives a turbid appearance and an unpleasant caustic taste to the preparation. The Liquor calcii chloridi of the old London Pharmacopœia consists of a solution of ℥jv. of the *fused*, or anhydrous chloride of calcium in fluid ℥xij. of distilled water. In the Edinburgh Pharmacopœia the Solutio calcis muriatis is composed of ʒviij. of the *crystals* of the salt in fluid ℥xij. of distilled water. The Aqua calcis muriatis of the Dublin Pharmacopœia holds two parts of the *dry* muriate of lime in seven parts of water. Neligan, in the fourth edition of his Materia Medica, made the strange mistake of stating that the Edinburgh officinal solution is two and a half times the strength of the Dublin preparation, for-

getting that the latter is made of the dry or anhydrous salt and the former of the crystallized or hydrated salt, the water of crystallization of course almost equalizing the proportions. I think the old Edinburgh pharmacopœial solution will be found at present the most suitable, and it is certainly the most palatable. The dose for an adult should vary from twenty or thirty to fifty minims in a wine-glassful of milk three times a day or oftener, according to the nature of the case, and should be taken, if possible, after food. When there is sickness or irritability of the stomach and bowels, the smaller dose more frequently repeated, and the vehicle warmed, will be found advantageous. Cod-liver oil is of course as valuable a companion to the chloride of calcium in appropriate cases, as it is to the iodides.

Milk is in every respect the best, as indeed it should be, regarded as a cardinal element in the dietary of such cases as indicate the use of the chloride. I am inclined to think that when the solution of the chloride of calcium is put into the milk, some chemical decomposition takes place under the influence of the lactic acid present. At all events a slight odour of chlorine becomes perceptible in the mixed liquids, which does not present itself in the officinal solution of the salt. Next to milk, syrup is the best medium, and bearing in mind the vastly augmented solubility of lime in the presence of sugar, I would venture to suggest the adoption for general use of a syrup of chloride of calcium composed of ℥v. of the crystallized salt in fluid ℥xij. of syrup, of which the full dose would be a teaspoonful, which of course could be as readily given in milk. —*Practitioner*, *Oct.* 1877, *p.* 247.

91.—ON ANÆSTHESIA AND ANÆSTHETICS.

By T. Spencer Wells, Esq., F.R.C.S.

In 1872 I made known my opinion that all the advantages of complete anæsthesia, with fewer drawbacks, can be obtained by the use of bichloride of methyline or chloromethyl than by any other known anæsthetic. That was the result of an experience of five years, and of 350 serious operations. The experience of the five succeeding years up to the present time, with more than 600 additional cases of ovariotomy, and many other cases of surgical operations, has fully confirmed me in this belief. Given properly diluted with air, the vapour of chloromethyl has, in my experience of ten years with more than 1000 operations of a nature unusually severe as tests of an anæsthetic, proved to be without a single exception applicable to every patient, perfectly certain to produce complete anæsthesia, relieving the surgeon from all alarm or even anxiety ; and its use has never

been followed by any dangerous symptom which could be fairly attributed to it. I wish I could speak as confidently of the chemical composition of the fluid sold as bichloride of methyline as I can of its anæsthetic properties. But whatever may be its chemical composition, whether it is or is not chloroform mixed with some spirit or ether, or whether it really is bichloride of methyline, I am still content with the effects of the liquid sold under that name, when properly administered. The only deaths ever attributed to it were, I believe, rather due to asphyxia. No air was given with the methyline. By Junker's apparatus, air charged with methyline vapour is given—not the vapour itself—and, so employed, it has always been in my experience both efficient and safe. I am sorry that some of the analytical chemists whom I have asked to clear up the question of its composition have not done so. It ought to be done, it can be done, and it must be done.—*Lancet, Aug.* 11, 1877, *p.* 191.

92.—SUBCUTANEOUS SYRINGE CASE.

Messrs. Salt and Son, Birmingham, have introduced a very elegant little subcutaneous syringe case made in aluminium. The case is neatly eugraved, with shield for crest or monogram; it contains a thoroughly well-made syringe, graduated on the piston rod, with stop screw to regulate the amount of fluid injected, two bottles for morphia, &c., capped and stoppered. The case closes securely with a snap, similar to a lancet case, and may safely and conveniently be carried in the pocket. Its external appearance is elegant.

93.—ON HYDROBROMIC ACID IN TINNITUS AURIUM.

By Dr. Edward Woakes, Surgeon to the Throat Hospital.

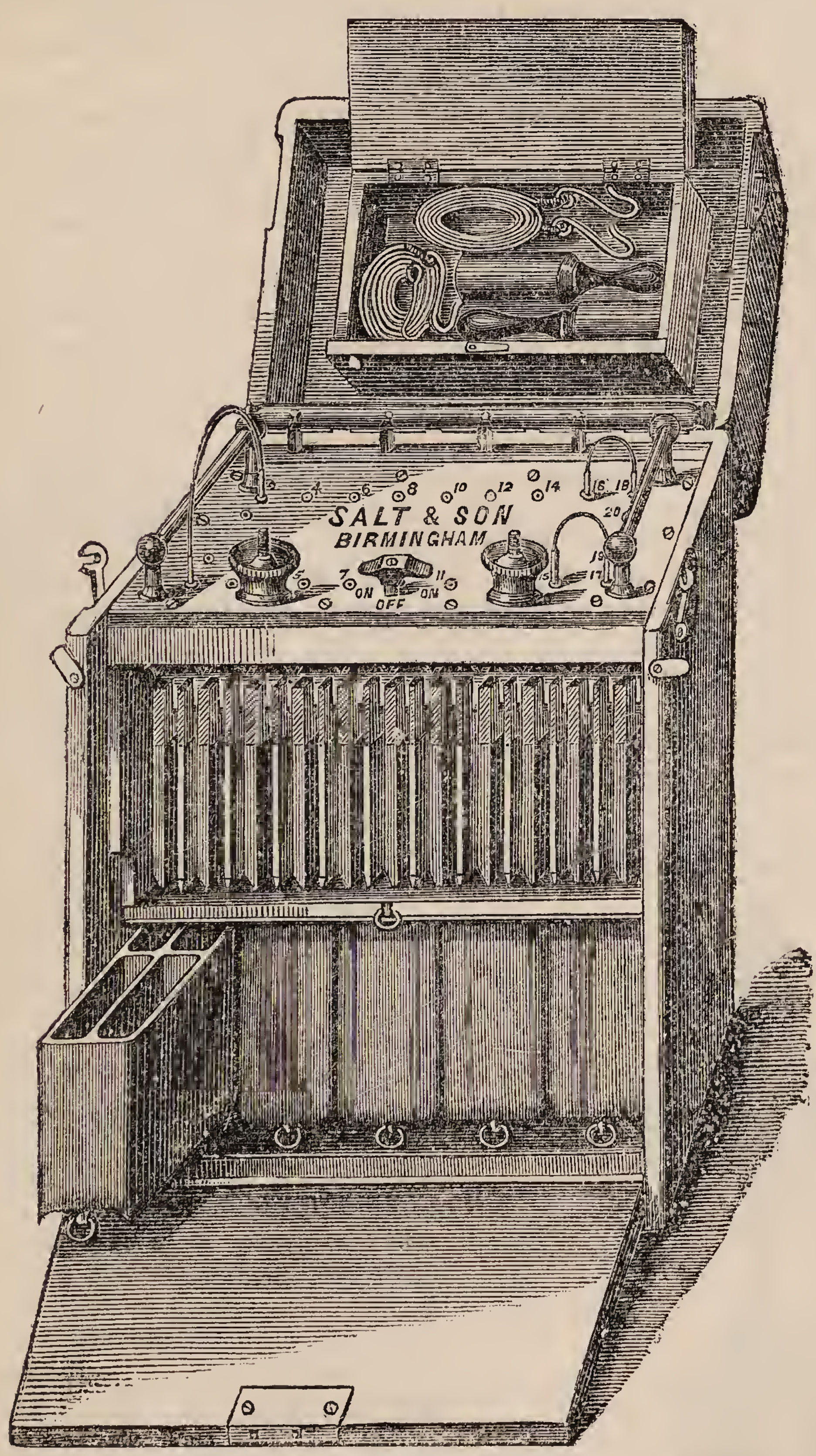
This drug having established its claim to antagonise the ear-symptoms occasioned by large doses of quinine, there appears to be but one step between this fact and the inference that it should be equally efficacious in analogous states of the ear arising from other causes. Viewing certain forms of tinnitus as possessing marked analogy to the condition induced by quinine—one, that is, of congested labyrinthine circulation—I have prescribed certain remedies with a view to the relief of this most distressing symptom; amongst these codeia, with some advantage, but not in any degree comparable to the results attending the hydrobromic acid. It may be needless to remark that the cases should be selected with a view to their appositeness to the presumed physiological action of the drug; and the indication which should be regarded as most distinctly pointing in this direction is that the noises have more or less of a pulsating, or, as the patient will describe it, a "knocking" character. The existence of vertigo, if present, will rather confirm the indication for the exhibition of the acid. The subjoined cases are intended to illustrate these remarks, and are taken from a number of others under recent observation.

F. C., aged 24, was the subject of otorrhœa media, associated with tinnitus of a very distressing character. This latter symptom persisted long after the others had yielded to treatment. The patient, a fairly intelligent mechanic, described the noises as increased on lying down, when they became "like the knocking of his heart." He was ordered fifteen minims of hydrobromic acid in water every four hours. At his next visit, he stated that, after taking three doses, the noises had much diminished, an improvement which steadily continued, so that at the end of a week he considered himself well.

[In the next case related the hydrobromic acid was given, as in the previous case, with an equally rapid disappearance of all the symptoms associated with the tinnitus.]

Two points appear important to secure the success of the drug. 1. The auditory apparatus must be clear of any well marked objective morbid process. 2. The tinnitus should present the characters of congested blood-supply, already alluded to. In mentioning the foregoing facts at a recent discussion at the Harveian Society, owing to the lateness of the hour, the distinctive indications for the successful administration of the drug were not insisted upon, an omission which, I trust, this communication will sufficiently rectify.—*British Medical Journal*, *June* 23, 1877, *p.* 773.

94.—SALT AND SON'S NEW MEDICAL BATTERY.



This battery is the outcome of a series of experiments made by Messrs. Salt and Son, of Birmingham, to reduce to a minimum

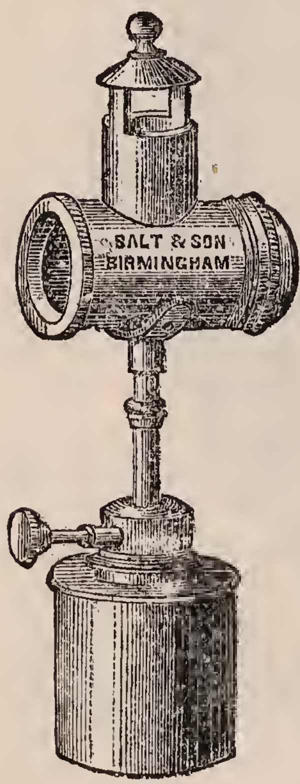
the risk of spilling the fluid contents of the cells whilst transporting the instrument to a patient's residence, and also to prevent the fumes from the acid injuring the plates by depositing a film upon them when in disuse.

The cells in this instance, twenty in number, and arranged in sets of four, are suspended on springs pressing upwards, but capable of being depressed *en bloc* by means of a hollow rod resting on the tops of the five stems used for drawing them up into contact with the plates.

The entire front of the case falls forward, and a wooden shelf, padded with vulcanised rubber, is made to slide between the tops of the cells and the bottom of the plates, at such a position that without touching the plates the cells are pressed tightly against it by the springs before mentioned, so that escape of the fluid is rendered impossible.

The cells themselves may be readily drawn out and removed for the purpose of emptying and recharging them. Another advantage consists in the fact that the whole of the mechanism is at once exposed, and is thus rendered amenable to the ready removal of any of the parts for repairs, amalgamation of plates, &c. All the parts are made so that any one can easily be removed by means of a screw sent with each battery. A measure in vulcanite accompanies each instrument, containing the exact quantity of fluid required for each cell when the plates are engaged.—*Lancet, Sept.* 29, 1877, *p.* 467.

95—NEW ILLUMINATING LAMP FOR MEDICAL PURPOSES.



Messrs. Salt and Son, of Birmingham, have brought under our notice a new Illuminating Lamp for medical purposes, the flame of which burns with a steady light, its intensity being increased by means of a reflecting mirror, whilst the rays are concentrated by a biconvex lens, so that they may be directed on any given point. No shade is required, and the flame being inclosed in a lantern, is not affected by currents of air. The wick is fed by a sponge saturated with petroleum.—*Medical Times and Gazette, Sept.* 15, 1877.

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